A COMPARATIVE STUDY OF THE LEVEL OF ASPIRATION OF NORMAL AND OF NEUROTIC PERSONS

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I. INTRODUCTION

Knowledge of the goal a person sets himself is important for the true appraisal of the efficacy of his behaviour and for the understanding of his reactions to environmental stress. Such goals may be explicit and formulated by the individual himself who announces, for instance, his intention of running a certain number of miles in a given time; or the formulation may come from the environment which prescribes a standard of achievement which the individual has to attain. Alternatively, the goals may be implicit, unformulated, and discovered only by studying the individual's subjective experiences of success and failure when he performs tasks of varying difficulties.

A comparison of the goals of different individuals and in particular of normal and of neurotic persons may thus prove a useful approach to the understanding of their personality make-up. One of the factors in a neurotic condition is frequently the discrepancy that exists between the goals or demands of the individual and the opportunities afforded by his make-up and by the environment to achieve them.

In view of the difficulty of comparing directly the principal goals in life of individuals, the problem had to be narrowed down to enable objective measurement. This was done by means of tests of level of aspiration.

Hoppe (1) originated the level of aspiration technique when he carried out a systematic study of the goal of individuals within a miniature situation. He gave his subjects a wide variety of simple tasks in a relatively free situation and inferred success and failure experiences from spontaneous utterances and general behaviour. Hoppe found that success and failure did not depend solely upon the objective quality of the performance, but upon the individual's "*Anspruchsniveau*" or level of aspiration. He defined the level of aspiration as "a person's expectations, goals or claims on his own future achievement in a given task". On the basis of his experiments, Hoppe found that a given performance is accompanied by a feeling of failure if it falls below the level of aspiration and vice versa. Hoppe's main measure of the height of the level of aspiration was the level of performance at which success was experienced. By this technique, Hoppe was able to formulate certain general tendencies that he observed regarding level of aspiration behaviour. However, within the framework of these tendencies, he found wide individual differences, and this led him to stress the value of the technique in the assessment of personality.

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Hoppe's evaluation of the level of aspiration behaviour was essentially qualitative depending upon the observer's ability to assess the individual's experiences of succes and failure. An advance in the study of the level of aspiration behaviour was made b Frank (2), who introduced a quantitative method of evaluation. He too used simple task which were repeatedly carried out. Goodness of performance was measured in terms o the time taken to complete the set task. The subject was acquainted with the task, give a number of trial runs, and after each trial told how long he had taken. He was sub sequently asked to state how long he thought he would take to complete the task nex time. At the end of that trial, he was again told his score and asked to state his nex 'aspiration'. By this method, the goal was verbalized and expressed in the same unit in which the goodness of performance was measured, thus enabling direct comparisor to be made between the level of aspiration and the level of performance. Frank altere Hoppe's concept of level of aspiration behaviour in the light of this quantitative techniqu to "the level of performance in a familiar task which an individual explicitly undertake to reach".

Frank used two principal scores to evaluate level of aspiration behaviour: (1) The goa discrepancy score, which is the difference between the previous performance and th subsequent goal; it is positive, if the goal is higher than the previous performance, an negative, if the goal is lower than the previous performance. The average goal discrepanc score of an individual is based upon the mean discrepancy score of the separate trial (2) The percentage score of typical reactions to success and failure, or the responsivenes of the level of aspiration to changes in the level of performance. This is based upo Hoppe's observation that the goal tends to be raised after success and to be lowered aft failure. Responsiveness is thus the total number of times that the level of aspiratio follows the immediately preceding performance in an upward or downward directio plus the number of times that the level of aspiration remains unchanged when the lev of performance is unchanged out of the total number of responses made.

Following the pioneer work of Hoppe and Frank, many subsequent investigations level of aspiration behaviour have been carried out in the last decade. These includ investigations of the reliability of the technique by means of the split half reliabilit method and by retest after an interval. The split half reliability coefficients were in th neighbourhood of +0·95 (3), whilst the reliability coefficients for retest after a wee ranged in Frank's (2) investigation from +0·57 to +0·75.

Generality of the behaviour has been examined by several psychologists (2, 3, 4, 5); th correlations were all positive and such as to warrant Frank's description of the behavio as a "relatively permanent characteristic of the personality" (2). This generality is n absolute and is affected to varying degrees by situational factors, by similarities of th abilities tested and of the methods of scoring used, as well as the amount of success an failure experienced. Himmelweit (6) carried out an investigation of the generality of th behaviour by giving two different tests to forty male neurotic patients on successive day The two tests used were an adaptation of the pursuit meter and a test of digit-symb substitution. The insignificant correlations obtained between the performance scores of th two tests suggest that the tests involve different abilities. The correlations between th various discrepancy scores were all positive (probability of these occurring by chan being less than 1 : 100). The correlation coefficient for the goal discrepancy scores wa +0·252, that of responsiveness to success and failure, +0·450. These figures were som

what lower than those of Frank(2) and more in line with the ones obtained by Gould(4). Frank found correlations of +0·50 to +0·69 when he tested twelve students with different tasks on the same day. Gould gave six different tasks to her subjects and obtained a median correlation coefficient of +0·29. She found that the correlations were significantly reduced when the tests were given after an interval of a few days. Allowing thus for a decrease in the generality of the behaviour when the abilities measured are unrelated and when the tests are given on different days, it appears that the level of aspiration behaviour of the neurotics is no more variable than that of normal subjects. However, no comparative investigation on this point has been carried out.

Attempts have been made to analyse the dynamics involved in level of aspiration behaviour. According to Frank(7) "the level of aspiration represents the final integration of complex and constantly shifting personal and situational factors". Different investigators have attempted to arrive at a clearer understanding by isolating and controlling some of the factors involved. Frank(8) and McGehee(9) have shown that the goal the individual sets himself is not simply an intellectual estimate of his performance but that conative factors play a part in determining the estimate. Hoppe(1) and Frank(10) postulated that the verbal statement of a goal is the resultant of a conflict or choice between the realistic judgement, the desire for self-protection, and the desire for gratification of one's self-esteem or "*Ichniveau*". The judgement factor would tend to approximate the goal to the actual performance, the other two factors working to distort this judgement. Of these two factors, one is the hedonistic factor (Hoppe's terminology), which would tend to lower the goal so as to increase the opportunities for exceeding it at the subsequent trial. The other factor, however, works in the opposite direction; it tends to push the goal up and is bound up with the opinion the individual has of himself. The preponderance of either of these two factors over the judgement factor results in a deviation of the level of aspiration from that of performance in either a positive or negative direction.

The extent to which any of the three tendencies mentioned above will operate, will largely depend upon the degree of 'ego-involvement'(11). By ego-involvement, Sears(11) understands the extent to which an individual considers that his performance is an expression of his own worth. This concept is a very useful one, and the various factors producing a given level of aspiration can best be studied in relation to it. The strength of the ego-involvement will depend in the main upon the interplay of two factors: (*a*) the environmental factor or reference scales against which the individual evaluates his performance and (*b*) the personality make-up of the subject.

Such reference scales may be artificially produced by the experimenter, who tells the subject his score in terms of the average performance of different groups, some of which the subject would consider to have high, others to have low prestige value. Chapman & Volkmann(12) and Festinger(13) on adults, and Anderson & Brandt(14) on children, demonstrated the influence of these reference scales upon the size of the goal discrepancy scores. Other reference scales are internalized, i.e. they are the standards the individual uses to evaluate his own performance without any suggestion on the part of the experimenter; the standards depending upon the individual's opinion of his position in the group, and upon his past experiences of success and failure. Sears's(11) very illuminating study of successful and unsuccessful schoolchildren points to significant differences in their level of aspiration behaviour. These various investigations show that subjects who are either

superior to the average of their own group or to that of a fictitious group with which they are compared, have a relatively low goal discrepancy score (predominance of the judgement factor), and show a high degree of responsiveness in their levels of aspiration to changes in the level of performance. Conversely, subjects whose achievements are below the average of their own group or that of a fictitious superior group, have a higher positive goal discrepancy score (predominance of the factor of self-esteem), and are less responsive in their goal setting to changes in the level of performance. Moreover, their standard deviations as a group are larger, their goal-setting behaviour being less uniform than that of the 'success' group. Previous experiences related to success and failure in a wider field thus determine in part the scores in a level of aspiration test, but do not account for the total variance. It seems reasonable to suppose that the personality make-up of the individual constitutes an important factor.

Attempts at correlation between different levels of aspiration and personality traits were made. Insignificantly positive correlations were found between goal discrepancy scores and scores obtained on Maslow's Social Inventory and on Guilford's Extraversion-Introversion Questionnaire(15). Sears's study was more suggestive of a relationship between level of aspiration scores and certain personality variables. He selected from the children tested those who showed either a consistently high positive, low positive, or low negative discrepancy score pattern. Comparing the groups for neurotic traits, he found significant differences. The low positive group was found to contain the highest number of well-adjusted children, whilst the majority of children in the high positive and the low negative groups exhibited tension and anxiety. These results have been corroborated by Rotter's(16) study of criminals.

The present investigation is a further attempt to obtain information about the personality characteristics that are bound up with goal-setting behaviour. The investigation differs from that of Sears by testing clearly defined groups which are known to differ either with respect to ability to adjust to environmental stress or with respect to certain important personality characteristics. In view of the emphasis that psychiatrists place upon the discrepancies that exist between the aims of neurotics and their actual performance, it was thought that a comparative study of goal-setting behaviour in relation to performance of normal and of neurotic subjects would be of some value. The study is divided into two parts: Part I reports on a differential study of the goals of normal and of neurotic men and women, and Part II gives the results of a more detailed inquiry into the goal-setting behaviour of persons suffering from hysterical and from dysthymic disorders.

II. Experimental investigation. Part I

(1) *The experimental population*

The experimental population consisted of twenty normal men and thirty-three normal women, and of a hundred neurotic men and thirty-two neurotic women. Attempts were made to equate the normal and the neurotic groups, as far as possible, for age, social, and educational background, in order to bring out more clearly the personality differences between the groups.

The neurotic groups consisted of non-commissioned members of the Armed Forces between the ages of 18 and 40 who had been sent to Mill Hill Neurosis Centre for diagnosis and treatment. They had been sent to the hospital because of difficulties in adaptation

to the exigencies of army life; the maladjustment showed itself objectively in a decrease of efficiency at work, and subjectively, in a variety of symptoms. The degree of neuroticism varied considerably. Some of the subjects were able to return to duty after a course of treatment lasting a few weeks, others were discharged and required treatment subsequently at a civilian neurosis centre. In view of the difficulty of comparing the extent of maladjustment of one patient with that of another, no attempt to rank the subjects in order of severity of their neurosis was made. The subjects represented a characteristic sample of persons suffering predominantly from hysterical and dysthymic disorders.[1]

No patients suffering from psychoses or from organic disorders were included.

The normal group of men consisted of privates and non-commissioned officers of the Royal Corps of Signals and of a clerical unit. They differed widely in the type of posts they held, e.g. they were cooks, technical staff, drivers, clerical workers, etc., as well as in their social background and past experiences. In this divergence in background and experience, they were comparable to the neurotic group. No rating on their efficiency at work could be obtained.

It was not possible to obtain a strictly comparable control group for the female Service patients. Nurses in the hospital of comparable age range were taken. They represented a control group in so far as they carried out their work efficiently and showed no evident signs of maladjustment. There may, however, be a greater temperamental uniformity amongst the control group, who were all of the same profession, than amongst the female neurotic group.

(2) *Experimental procedure*

A review of the tasks chosen by other investigators showed the importance of using tests that satisfied the following criteria:

(1) The method of scoring should be unidimensional to enable the subject to appreciate readily any improvement or deterioration in his performance.

(2) The tasks should not be associated with a specific occupational proficiency and thus introduce a bias for some subjects.

(3) The task must be within a given range of difficulty. No ego-involvement occurs when tasks are either too easy or too difficult(1).

(4) The test should allow for improvement on successive trials.

(5) Successive scores should show moderately high intercorrelations to make approximate anticipation of future scores possible.

(6) The test should possess a certain intrinsic interest value.

The apparatus finally selected was the Triple Tester, designed by the late Dr Craik of the Cambridge Psychological Laboratory. It is an adaptation of the pursuit meter, and consists of a brass drum carrying an Ivorine cover. The drum rotates at a fixed speed. The Ivorine cover is marked out as a helical 'road' with holes punched in it. A 'vehicle' in form of a bronze ball is steered along this road by a steering wheel. The task consists in keeping the ball on this line of holes and in touching as many holes as possible, each contact being automatically recorded on an electric counter. The steering wheel operates the rack through an integrating gear instead of directly; this renders the task more difficult and produces the desired variability in scores and in the rate of improvement.

The subject was familiarized with the apparatus and given a few trial runs. He was told to aim at hitting as many holes as possible. After being informed that the maximum possible score was 220, he was asked how many he thought he would hit at the next trial. If he hesitated, he was encouraged to 'guess' and was told that in the course of the test he would have opportunities for making more appropriate judgements. The figure stated was noted, and the subject began the test. At the end of the trial, he was asked to state how many holes he thought he had actually hit and was then informed of his score. Before the next trial he was again asked: "How many do you think you will hit next time?" This pro-

[1] Dysthymia is a collective term embracing disorders such as anxiety states and depressions with or without obsessional features(21).

cedure was repeated ten times so that ten estimates of his future performance ('aspirations'), ten estimates of his past performance ('judgements'), and ten performance scores ('performances') were obtained. It will be seen that in the instructions, emphasis was placed upon intellectual judgement of future performance rather than upon a desired goal. It was thought that deviations from a judgement obtained in this manner would be psychologically more significant.

This procedure differs from that used by Frank(2) in that the estimates of past performance were also obtained. Hilgard & Sait(18) had found that distortions occurring in self-estimates with respect to past performance were correlated positively with distortions in such estimates which referred to future performances.

(3) *Results*

The results can be divided into those relating to the actual performance on the test and into those relating to the level of aspiration behaviour of the subjects.

Table 1 gives the performance score averages of the four groups.

Table 1. *Performance score averages*

	Male control group	Male neurotic group	Female control group	Female neurotic group
Mean	126·25	69·74	64·68	30·34
Sigma	28·25	30·43	17·06	18·09
Coefficient of variation	22·22	43·33	22·75	60·02

The performance scores of the women were very significantly lower than those of the men; this applied to both the normal and the neurotic groups. It is probable that the mechanical nature of the test introduced a bias in favour of men. The average scores of the neurotics were very much lower than that of their respective control groups (C.R. 8·07 for the men and C.R. 8·23 for the women). As a comparison of the standard deviations would be meaningless in view of the great difference in means, the standard deviations were transformed into coefficients of variation. It was found that the coefficients of variation were considerably larger for the neurotic than for the normal groups. The results are in line with those obtained on a test of speed and accuracy where the normal groups had a significantly higher performance score and showed a significantly smaller interpersonal variability than the neurotic groups (Himmelweit(19)).

Whilst performance scores can be evaluated in themselves, aspiration and judgement scores have to be related to performance scores to become meaningful. The level of aspiration behaviour of the subjects was studied on the basis of the following measurements:

(1) *The average goal discrepancy score* is based upon the difference between the previous performance and the aspiration for the next trial. It is positive if the aspiration is higher than the preceding performance, and negative if it is lower. As the first trial was discarded, eight discrepancy scores in all were obtained.[1]

(2) *The average judgement discrepancy score* is based upon the difference between the performance and the estimate of the goodness of that performance; underestimation of one's past performance gives a negative score, overestimation a positive score. Since the first judgement was made after the subject was acquainted with the test, it was not discarded and ten judgement discrepancy scores were therefore obtained.

[1] Other investigators calculated the average attainment discrepancy score; it is the difference between the estimate of the performance and the actual performance; when the performance is higher than the estimate of that performance and vice versa. It was found that goal and attainment discrepancy scores measure virtually the same factors, since the intercorrelation for the men was $-0{\cdot}934 \pm 0{\cdot}031$, and for the women $-0{\cdot}943 \pm 0{\cdot}021$ (6). The inverse relationship is due to the manner in which the scores were derived.

(3) *Index of flexibility.* This is the number of shifts of the level of aspiration based upon a maximum of nine shifts, the first trial being discarded. Previous investigators had shown that subjects differed in their readiness to adjust the level of aspiration to changes in the level of performance.

(4) *Percentage of typical reactions.* Since the index of flexibility does not take into account the direction of the changes of the aspiration score, the percentage of typical reactions was worked out on the basis of Hoppe's findings that the aspirations tended to be raised after success and lowered after failure. The percentage of typical reactions is, therefore, the percentage of "normal" reactions to success and failure out of the total number of reactions observed.

Table 2 gives the various measures of goal-setting behaviour for the four groups. It will be seen that each group tended to overestimate the future performance (positive goal discrepancy score), and tended to underestimate the past performance (negative judgement discrepancy score).

Table 2

		Male control group	Male neurotic group	Female control group	Female neurotic group
Goal discrepancy score:	Mean	+10·72	+10·38	+2·26	+6·15
	Sigma	6·33	14·32	4·98	6·62
Judgement discrepancy score:	Mean	− 4·29	− 1·17	−7·11	−3·50
	Sigma	6·08	9·49	5·76	7·25
Index of flexibility:	Mean	4·25	4·32	5·69	5·73
	Sigma	0·71	1·83	0·54	1·02
Percentage score of typical reactions:		45	49	48	49

Certain very significant sex differences were found which applied equally to the normal and the neurotic groups. The goal discrepancy scores of the women were significantly smaller than those of the men. The women overestimated their subsequent score to a lesser degree, whilst at the same time they underestimated their past performance to a more marked degree than the men (higher negative judgement discrepancy score). In addition, both normal and neurotic women had a significantly higher index of flexibility; they adjusted their goals more readily to changes in performance.

The level of aspiration behaviour of the neurotics differed significantly from that of the normal subjects. Normal men will be compared with neurotic men and normal women with neurotic women. The difference in average goal discrepancy scores was significant for the women only (C.R. 2·66); the neurotic group tended to overestimate their future performance more than the normal group. The underestimation of past performance was more moderate for the two neurotic groups than for the normal groups; the difference was significant both for men and women (C.R. 2·01 and 2·08 respectively). No significant differences in the secondary measures of level of aspiration behaviour were found between neurotics and normals.

The normal and neurotic groups differed very significantly with respect to the size of their standard deviations, those of the normal groups being smaller. The critical ratios for the difference in the size of the standard deviations for the goal discrepancy scores were 4·01 and 2·01, and for the judgement discrepancy scores 2·52 and 1·23, for men and women respectively.

To arrive at a clearer understanding of the factors producing a given level of aspiration behaviour, the interrelationship of the various measures was examined. The intercorrela-

tions were calculated separately for the four groups (Table 3). Interesting differences in the patterning of the responses for the neurotic and for the normal groups were obtained. Whilst a significantly positive correlation for goal and judgement discrepancy scores was observed in the case of the normal groups, the correlations were significantly negative

Table 3. *Intercorrelations of various level of aspiration measures*

	Male control group	Male neurotic group	Female control group	Female neurotic group
Goal and judgement discrepancy	$+0{\cdot}591 \pm 0{\cdot}130$	$-0{\cdot}682 \pm 0{\cdot}050$	$+0{\cdot}391 \pm 0{\cdot}151$	$-0{\cdot}394 \pm 0{\cdot}152$
Goal discrepancy and typical reactions	$-0{\cdot}251 \pm 0{\cdot}201$	$-0{\cdot}591 \pm 0{\cdot}072$	$+0{\cdot}132 \pm 0{\cdot}163$	$-0{\cdot}273 \pm 0{\cdot}155$
Judgement discrepancy and typical reactions	$-0{\cdot}531 \pm 0{\cdot}151$	$+0{\cdot}348 \pm 0{\cdot}092$	$-0{\cdot}222 \pm 0{\cdot}173$	$+0{\cdot}363 \pm 0{\cdot}151$

in the case of the neurotic groups. This opposite relationship applied to both men and women; the difference was very significant in the case of men (C.R. 3·83), and suggestive in the case of women (C.R. 1·41). Amongst normal subjects, those who overrated their future performance more than others, tended to underrate their past performance to a lesser degree than other subjects. Conversely, those subjects who overrated their future performance to a moderate degree, underrated their past performance considerably.

In the case of neurotic subjects, those who overrated their future performance more than others, at the same time underrated their past performance to a more marked degree; whilst those who showed a moderate overestimation of their future performance showed a similar moderate underestimation of their past performance.

The signs of the correlations between judgement discrepancy scores and percentage of typical reactions were negative for the normal groups, and positive for the neurotic groups (C.R. 1·99 for men and 1·01 for women). Amongst normal subjects the tendency to underestimate their achievement was correlated with ready adjustment of their goals to changes in performance. In the case of neurotic subjects, underestimation of past achievement was correlated with a higher percentage of atypical reactions to success and failure.

(4) *Discussion of Part I*

A study of the scores of the four groups tested revealed two facts applicable to them all. In the first place, all subjects overestimated their future score and, in the second place, all underestimated their past achievement. In addition, it has been shown that overestimation of future performance and underestimation of past performance were interrelated, requiring an explanation in terms of some common factor. The patterning of the goal and judgement discrepancy scores differed for the normal and for the neurotic groups; an interpretation of the normal pattern will first be given against which to evaluate the neurotic responses.

The subject tended to set himself a goal ahead of his performance, over and above any probable improvement that he might achieve at his next trial. His level of aspiration is thus not simply an intellectual estimate, but an estimate that has been coloured by the desire to do well (Hoppe's *ego-niveau*). This must be understood in terms of the society in which we live. In our present-day competitive society, throughout childhood and adolescence, emphasis has been placed upon aiming at a state of knowledge or of achievement just ahead of that which the individual has mastered. It has thus become an interiorized mental set for the individual to work with a goal somewhat ahead of his performance and

this affects his estimate of future improvement. Having thus set himself a goal which is in part an estimate of the probability of future improvement of his score and in part the result of a wish to do well, the subject completes the actual trial. The performance exerts a sobering effect and, in his effort to keep in touch with the reality situation, the subject overcompensates for his previous 'optimistic outlook' by underestimating his actual achievement (negative judgement discrepancy score). The positive correlation between goal and judgement discrepancy scores suggests that such overcorrection occurs within certain limits. The person who considerably overestimates his future performance reacts to this overestimation to approximately the same degree as the person who only moderately overrates his future performance. In the former case, this leads to a slight underestimation, in the latter case, assuming a similar correction to take place, the underestimation becomes more pronounced.

Judgement discrepancy scores must, therefore, be viewed, not only in relation to the actual performance, but also to the goal that the individual sets himself. Such relationship is, of course, not an absolute one, and it is justifiable only to speak of a tendency on the part of the subject to react in this manner. These findings confirm those of Hilgard & Sait (18). They tested fifty college students on a card-sorting and on a pursuit-learning test. In both instances, the correlations between goal and judgement discrepancy scores were positive (+0·29 for card sorting and +0·77 for pursuit learning). As Hilgard & Sait pointed out, these correlations were especially significant in view of the fact that the abilities involved in the two tests were unrelated. The interpretation of the relationship between these two scores given by Hilgard & Sait differs, however, from the one outlined above.

The goal-setting behaviour of the women is less pronounced than that of the men. Their goals tended to be placed very slightly above their performance scores. If the extent of overcompensation is assumed to be approximately the same for men and women, this would account for the considerably larger underestimation of past performance by women. An additional factor may, however, lie in the particular temperamental make-up of the female control group. Presumably their choice of profession has been in part determined by certain characteristics common to them all. It would, therefore, be interesting to repeat the experiment with a control group of a more varied background and personality make-up.

It has been shown that the patterning of the responses of the neurotic groups differed from that of the normal groups. Those neurotics who overrated their future performance considerably, at the same time underrated their past performance to the same marked extent, whilst those who showed a moderate overestimation of their future performance showed a similar moderate underestimation of their past performance. This is best shown graphically. In Fig. 1, two male controls, the first with a relatively high, the second with a moderate goal discrepancy score, were matched against two neurotics with the same goal discrepancy scores in order to demonstrate the relationship between goal and judgement discrepancies for normals and for neurotics respectively.

The signs of the correlations between judgement discrepancy scores and percentages of typical reactions were opposite for the normal and neurotic groups. In normal subjects, underestimation of past performance was compatible with a normal reaction to success and failure, whilst in the same circumstances neurotic subjects tended to give atypical reactions.

The spread of scores of the neurotics was very significantly wider than that of the normal subjects. The distribution of the scores of neurotics was somewhat bimodal. We are dealing, therefore, with two main types of neurotic behaviour. The one gave relatively large deviations in both goal and judgement from the actual performance and was rigid in making these estimates; the other exhibited very moderate deviations from the level of performance in estimates of past and future achievement. These responses were

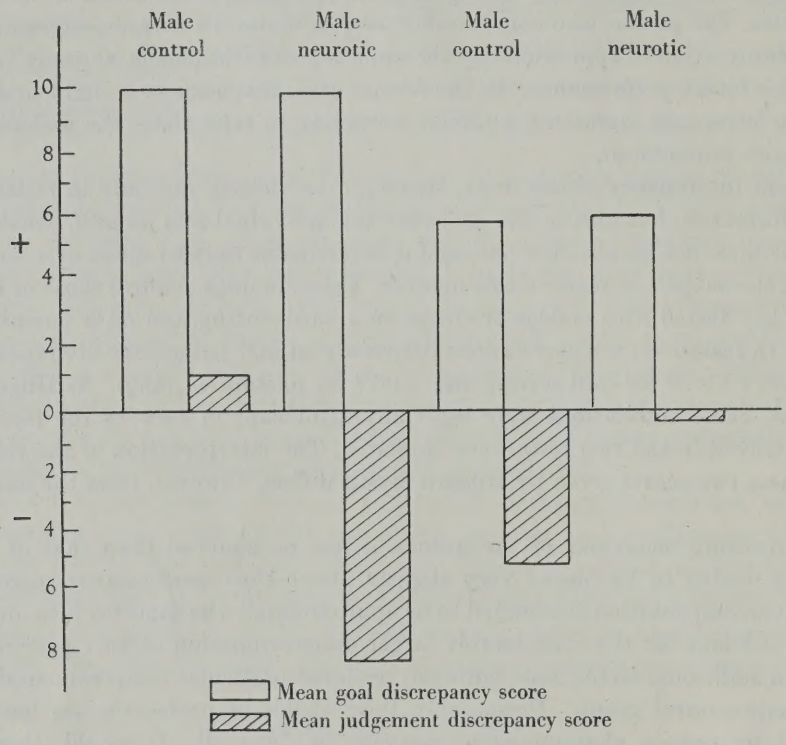

Fig. 1

'realistic' and flexible. In a previous study of Eysenck & Himmelweit (20), light has been thrown upon the personality make-up of these two types of neurotic responses. These will be discussed in Part II together with evidence which has since been obtained.

At first sight, it appears that the differences in the two types of neurotic behaviour, namely the primarily 'objective' and the primarily 'subjective', correspond to differences in degree of neuroticism. Analyses of the case papers of the hundred neurotics, and in particular a comparative study of the percentage returned to duty and discharged from the army in either group, showed that there was no significant difference in the degree of neuroticism found in the two groups. Moreover, it has been shown on the normal population that subjective distortions of estimates of past and future performance, within limits, were compatible with good adjustment. Lack of these distortions as exemplified by the 'objective' neurotic is thus as much evidence of neurosis as the high distortions of the 'subjective' neurotic.

This comparative analysis of the behaviour of normal and of neurotic subjects showed that they differed with respect to the size of the deviations of their estimates of past and

of future performance and with regard to the patterning of the responses that constitute level of aspiration behaviour. It is probable that when other temperament tests are given to normal, to neurotic, and to psychotic subjects, differences in the patterning of the responses will emerge which will throw additional light upon the conative mechanisms of these groups. Investigations of the response patterns of psychotic subjects on level of aspiration tests are in progress.

III. Experimental investigation. Part II

(1) *Comparison of hysterics and dysthymics*

In a previous investigation (20), the differences in goal-setting behaviour and reactions to success and failure of hysterical and dysthymic patients were studied. These two types were selected for the following reasons:

(1) A factorial study of the case records of 700 male hospital patients had led to the isolation of a bipolar factor which was found to correspond to hysteria and to dysthymia respectively (21).

(2) These two types of patients had shown significant differences in their responses to a number of conative tests such as persistence (22), speed and accuracy (19), etc.

These patients were also selected in order to investigate experimentally certain hypotheses regarding goal-setting behaviour. These were formulated on the basis of Jung's (23) description of the hysterical and the neurasthenic (dsythymic) personality make-up. Jung, and later McDougall (24), considered the hysteric to be the prototype of the extravert, and the neurasthenic the prototype of the introvert. They maintained that persons of the introvert temperament, under strain, are more liable to disorders of the neurasthenic type and persons of the extravert temperament to disorders of the hysterical type. One of the main differences between an introvert and an extravert, according to Jung, lies in their approach to their environment. The introvert is described as subordinating object to subject; he is "*ichbetont*" in his appraisal of reality, whilst the extravert, on the other hand, tends to subordinate "das Subjekt dem Objekt...wobei dem Objekt der überragende Wert zukommt" (23).

This approach to the environment would mean, in terms of level of aspiration behaviour, that the hysteric, in contrast to the dysthymic, adjusts his estimates more readily to changes in performance and deviates less from the actual performance in his estimates of past and future achievement. The investigation was carried out on fifty male hysterical patients and fifty male dysthymic service patients at Mill Hill Emergency Hospital. The test used was the Triple Tester and the instructions were identical to those described in Part I. The results provided proof for the contention outlined above. The hysterics showed significantly smaller goal and judgement discrepancy scores and a significantly higher index of flexibility compared with the dysthymics. The level of aspiration behaviour of the hysterics was thus found to correspond to the 'realistic', flexible and 'objective' behaviour of the one type of neurotic described in Part I, and that of the dysthymic to the more rigid and 'subjective' behaviour of the second type.

Stagner (25) and others have pointed out that findings in the field of abnormal psychology are frequently of value in elucidating mechanisms which are operative in the field of normal psychology. If one accepts the theory that traits of human characteristics are distributed in the population in accordance with the normal unimodal curve, the goal-

setting behaviour of the hysterics and of the dysthymics would constitute the two extreme ends of the normal distribution curve. To test the validity of this contention, it appeared to be of value to ascertain whether the differences of behaviour recurred on a different type of test and whether they applied to women as well as to men. The present study, therefore, describes: (1) the behaviour of hysterical and dysthymic men on a test of a cognitive rather than of a mechanical type; (2) compares the behaviour of hysterical and dsythymic women with that of hysterical and dysthymic men; and (3) analyses more specifically some of the dynamics of the behaviour of these two types of neurotics under conditions of increased motivation.

(2) *The experimental population*

The experimental population consisted of thirty-four male dysthymic, twenty-nine male hysterical, thirty-five female dysthymic, and twenty-nine female hysterical service patients. All had been selected by the senior psychiatrists of the hospital as representing good clinical examples of the two disorders. Pure cases of either disorder are very rare, and the differences between the two groups are relative rather than absolute. Thus, it is true to say that the hysterical group consisted predominantly of patients suffering from hysterical symptoms and of a hysterical character make-up, with relative absence of overt anxiety, whilst the dysthymic groups were composed of patients with symptoms and make-up characteristic of anxiety states and depressive states, but with a relative absence of hysterical symptoms. Any differences that were found in the reactions of the two groups will be lessened by the impossibility of obtaining pure cases of either disorder, and the results will thus be attenuated to an unknown extent.

(3) *Experimental procedure*

For the present investigation, a symbol digit substitution test was designed.[1] The apparatus used was an adaptation of the Hollerith Punch. On this punch are ten keys which allow the punching of a card in ten different places. Each touch of the keys pushes the card a certain distance, exposing a new part to be punched. Forty-five movements can be made before the card is completely punched. This machine was adapted for use as a code symbol substitution test by (1) putting before the subject a card giving equivalent letters for the numbers appearing on the keys of the punch and (2) exposing automatically a certain letter whenever a given key is depressed. The subject would look at the letter shown, find the corresponding number on the card, and then press the key bearing the correct number which exposed the next letter. In this test, speed of performance was controlled by the subject. Each trial was limited to one minute and goodness of performance measured in terms of the number of holes correctly punched. The same procedure for obtaining the estimates of past and future performance was used as for the Triple Tester. Ten trials were given.

(4) *Results*

No differences between the performance score averages were found. The goal discrepancy scores of the dysthymics were higher than those of the hysterics (Table 4). The difference was significant for women (C.R. 2·29), but failed to reach significance in the case of the

[1] This test was selected because ability to perform on this test had been found to be unrelated to ability to perform on the Triple Tester(6). If differences in level of aspiration behaviour were found between the groups on tests differing in the abilities involved, it would be further proof that we are dealing with a generalized trait.

nen. When the patients were divided into groups according to diagnosis, irrespective of ex, and their critical ratios determined by using the Davies-Eysenck approximation ormulae(20), the difference was found to be statistically significant (C.R. 2·80). These esults are, therefore, in good agreement with those of the previous investigation(20). t is interesting to note that the discrepancy scores of the women with either disorder were maller than those of the men. It appears that we are dealing here with a sex difference ound with regard to normal and neurotic subjects (Part I).

Table 4

		Dysthymic men	Hysteric men	Dysthymic women	Hysteric women
Performance score average:	Mean	25·39	21·70	25·39	24·61
	Sigma	1·82	1·93	1·82	1·69
Average goal discrepancy score:	Mean	+3·23	+2·02	+2·82	+1·73
	Sigma	1·97	2·84	1·74	2·05
Average judgement discrepancy score:	Mean	+0·65	+0·78	−0·72	−0·85
	Sigma	1·85	1·63	1·76	1·67
		Dysthymics	Hysterics	Men	Women
Performance score average:	Mean	26·43	23·15	23·55	26·03
	Sigma	1·13	1·31	1·73	1·48
Average goal discrepancy score:	Mean	+2·94	+1·31	+3·03	+2·33
	Sigma	2·15	3·74	2·09	3·19
Average judgement discrepancy score:	Mean	−0·05	−0·86	+0·71	−0·81
	Sigma	1·92	2·35	1·73	2·29

The male patients showed a difference with respect to judgement discrepancy scores vhich was in line with that obtained on the Triple Tester. The difference was not statistic-lly significant. One possible explanation for this lack of discrimination of the judgement liscrepancy score between the groups lies in the set-up of the Punch Test. On the Triple Tester, an impression of the goodness of performance was derived from the number of licks heard each time the stylus made contact with a hole. The test was of two minutes' luration, and the scores ranged from 25 to above 100 points. Exact and realistic estimates of past performance were thus rendered difficult. A trial on the Punch Test lasted one ninute only, which allowed the subject to retain a clearer impression of his performance. Moreover, at the end of each trial, the subject gained an additional clue of the goodness of his performance from the position of the card in the carriage.

An index of flexibility of the various groups was obtained. The dysthymics were found o be significantly more rigid than the hysterics (C.R. 3·39). No sex differences were ound (Table 5).

Table 5

		Dysthymics	Hysterics	Men	Women
Index of flexibility:	Mean	4·33	5·45	5·06	5·31
	Sigma	2·18	1·60	1·79	1·38
Percentage of typical reactions		42	46	42	44

No difference in percentage score of typical reactions between the groups was found.

A comparison of the intercorrelations of the different measures of level of aspiration ehaviour on the Punch Test and on the Triple Tester was made. The correlations for the Triple Tester were those obtained from 100 male patients given in Part I. The correlations or the Punch Test were based upon the scores of all the four groups combined, i.e. upon total of 127 subjects (Table 6).

Table 6. *Intercorrelations of various level of aspiration measures*

	Punch Test	Triple Tester
Goal and judgement discrepancy scores	$-0{\cdot}293 \pm 0{\cdot}082$	$-0{\cdot}682 \pm 0{\cdot}054$
Goal discrepancy scores and typical reactions	$-0{\cdot}442 \pm 0{\cdot}073$	$-0{\cdot}591 \pm 0{\cdot}066$
Judgement discrepancy scores and typical reactions	$+0{\cdot}283 \pm 0{\cdot}084$	$+0{\cdot}348 \pm 0{\cdot}088$

The difference between the correlations was significant only in the case of the correlation between goal and judgement discrepancy scores. This was significantly smaller on the Punch Test than on the Triple Tester. An analysis of the correlations for the Punch Test show that the difference is due to a difference in intercorrelations for men and women. The judgement discrepancy scores for men and women differed very significantly with regard to the means and the distribution of scores (C.R.$_M$ = 3·48 and C.R.$_\sigma$ = 2·38). In view of this sex difference, the correlations for men and women were calculated separately (Table 7).

Table 7. *Goal and judgement correlation coefficients*

Men—No. 63	$r = -0{\cdot}531 \pm 0{\cdot}035$
Women—No. 64	$r = -0{\cdot}390 \pm 0{\cdot}106$

These correlations were compared with those obtained for men and women on the Triple Tester and no significant differences were found.

The above analysis shows that the components of level of aspiration behaviour that were studied operated in a similar manner on the two tests. The investigation has confirmed the findings of the previous study (20) on a test involving different abilities, and has provided experimental proof that the difference in goal-setting behaviour applies to both men and women suffering from hysterical and dysthymic disorders. The sex differences, irrespective of diagnosis, were similar to those obtained on the Triple Tester.

It might be argued that the difference is due to a difference in the degree of ego-involvement of the groups. The dysthymic is commonly described by psychiatrists as a person who distrusts his own abilities and considers every situation a challenge. No special stimulus is therefore needed to obtain his ego-involvement in a test, and in particular, in a test where the individual himself is the 'pace-maker' by setting the goal and the 'scorer' by evaluating his own performance. The hysteric, unlike the dysthymic, does not consider all situations a challenge of his worth, on the contrary, there are very few situations or tests where he would exert himself. Since Sears (11) has shown that the level of aspiration scores are affected by varying degrees of ego-involvement, it seemed important to increase the motivation of the hysterical subjects. Increased motivation was effected by the use of incentives.

(5) *Further experiments on motivation*

The experimental population consisted of twenty-two male dysthymics and of twenty male hysterical patients, selected in the manner described above.

The Triple Tester was used and the instructions given were identical to those described in Part I. In the first session, the patients were given ten trials in the usual manner. After a week, they were retested. Before starting the test, they were told that if they beat their previous best performance score by 30 points, they would be given 50 cigarettes or their equivalent value in money. By making the prize dependent on their own previous performance rather than upon some absolute standard, the effect of differences in ability was ruled out. The test then proceeded in the usual manner and ten aspirations, ten performances and ten judgements were obtained. The prize offered must be viewed in

relation to a private's pay and, as such, represents a considerable amount. That it acted as a true incentive could be inferred from the patients' spontaneous remarks and from the greater concentration shown by both groups in the second session. This was particularly striking in the case of the hysterics. Both groups were found to have improved in performance upon retest. The improvement was significant in the case of the dysthymics (C.R. 2·98) (Table 8).

Table 8. *Performance score averages*

	Dysthymics		Hysterics	
	Test I	Test II	Test I	Test II
Mean	71·83	100·15	73·08	94·74
Sigma	27·82	33·39	32·76	39·66
$C.R._M$	2·98		1·89	

Difference score
(Average score Test II minus average score Test I)

	Dysthymics	Hysterics
Mean	+28·32	+21·66
Sigma	25·36	14·11
$C.R._M$	1·16	

The data relating to the various measures of level of aspiration behaviour are given in Table 9 for Test I and Test II. This table gives, in addition, the difference scores for each group between Test I and Test II. It will be seen that the two groups did not differ significantly from one another on any of the scores. This is not surprising, in view of the

Table 9

		Dysthymics		Hysterics	
		Test I	Test II	Test I	Test II
Goal discrepancy score:	Mean	8·99	9·85	11·23	12·36
	Sigma	9·92	10·71	12·34	10·62
Judgement discrepancy score:	Mean	+0·33	−1·06	+1·88	+3·37
	Sigma	8·55	8·59	7·83	6·08
Goal discrepancy score (Test I–Test II):	Mean		−0·86		−1·13
	Sigma		12·23		16·10
Judgement discrepancy score (Test I–Test II):	Mean		+1·39		−1·49
	Sigma		8·49		9·10
Index of flexibility:	Mean	4·45	3·15	4·95	4·54
	Sigma	2·45	2·36	1·65	2·44
Percentage of typical reactions		47	48	47	49
Index of flexibility (Test I–Test II):	Mean		+1·30		+0·41
	Sigma		2·92		1·87
Percentage of typical reactions (Test I–Test II)			−1		−2

relatively small number of subjects tested, and in view of the considerable spread of scores that occurs in most tests of a conative type. However, certain interesting points emerge. First, under the stimulus of the reward, both groups tended to overestimate their future performance more than on the first test. This increase was more pronounced in the case of the hysterics, but not significantly so. It can be explained by our hypothesis that the dysthymic did not need incentives to be ego-involved, and his level of aspiration would not differ as much on Test II from that on Test I as that of the hysteric. Secondly, the dysthymics underestimated their own achievement more in the second test than in the first test, whilst the situation was reversed for the hysterics, who tended to overestimate their past performance more than on the first test. The average difference scores were worked out by subtracting the judgement discrepancy score of Test II of each subject from that of Test I. The average difference score for the dysthymics was $+1·39 \pm 8·49$,

that of the hysterics $-1{\cdot}49 \pm 9{\cdot}10$. Again in view of the small number of subjects, the difference was suggestive only (probability of 1 : 10). The difference in judgement discrepancy scores between Test I and Test II for the two groups is shown graphically in Fig. 2.

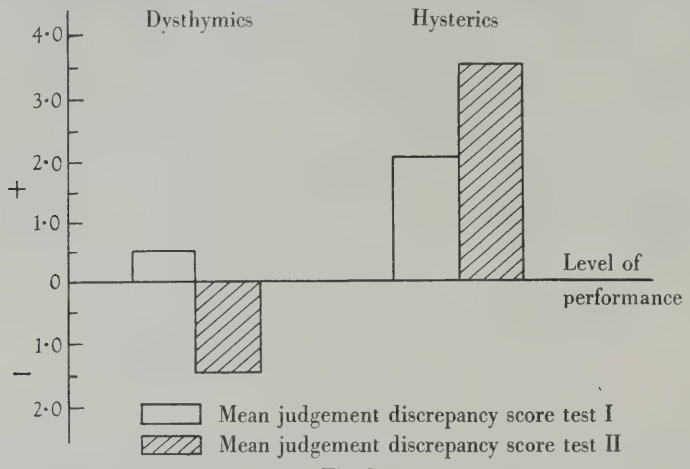

Fig. 2

Both hysterics and dysthymics showed a decrease in the index of flexibility under incentives. The decrease for the former was negligible, that for the latter highly suggestive (C.R. 1·80). No significant differences in the percentage of typical reactions to success and failure were found for the groups on either test (Table 9).

The introduction of incentives produced a heightened interest in the test which led to an increase in the size of the goal discrepancy scores (predominance of the desire to do well). It showed clearly that the differences observed on the Punch Test between hysterics and dysthymics could not be accounted for solely in terms of differences in the degree of ego-involvement, since increased motivation has accentuated the characteristic responses of these two types of patients.

(6) *Discussion of Part II*

The investigations reported here provide further evidence that hysterics and dysthymics differ in their goal-setting behaviour and in their attitudes to success and failure. It has been shown that these differences apply to both men and women and are found on tests of a predominantly mechanical type (the Triple Tester) and on tests of a more cognitive nature (the Punch Test).[1] Increasing the motivation of the subjects has led to an accentuation of the responses characteristic of these two reaction types.

Two hypotheses, which are complementary, can be put forward to account for these differences. The first is based on Jung's concept of the characteristic attitude of introverts and extraverts to their environment. The "*objektbetonte*" behaviour of the extravert would correspond to the 'realistic' and flexible goal-setting behaviour of the hysteric. The "*ichbetonte*" behaviour of the introvert, on the other hand, would correspond to the relatively 'irrealistic' and rigid behaviour of the dysthymic whose estimates of past and future achievements deviated widely from actual achievement.

[1] The correlation between performance score average and intelligence on the Triple Tester was $+0{\cdot}128 \pm 0{\cdot}103$ ($n = 100$), that on the Punch $+0{\cdot}327 \pm 0{\cdot}092$ ($n = 116$). The groups tested did not differ significantly in their intelligence test scores.

The second hypothesis enlarges upon the first by accounting for the responses of dysthymics and hysterics in terms of their predominant symptoms. Sears (11) in his study of successful and unsuccessful children, and other investigators of adults, found that a high goal discrepancy score was correlated with anxiety, insecurity, and generally with lack of self-confidence. One of the characteristics of anxious and of depressed patients is their lack of self-confidence. In conformity with previous findings, one would expect this lack of self-confidence to express itself in a high goal discrepancy score. The same lack of self-confidence led to a considerable underestimation when the subjects were asked to evaluate their own achievement. This has been brought out particularly clearly in the present series of experiments, where, under increased motivation, the dysthymic, anxious to do well, underestimated his actual achievement more than previously.

Sears's explanation accounts for the apparent contradiction between the fact that the anxious patient suffers from lack of self-confidence and yet 'expects' to do well on the subsequent trial. Sears and others have shown that subjects who find themselves below the performance of their group use the *goal* rather than the actual performance as a substitute goal response; they derive some gratification from setting themselves a high goal, while they would derive little or no satisfaction from beating their own goal, if it fell below that of the group. In Sears (11) and Anderson & Brandt's (14) investigations, the standards against which the subject evaluated his performance were either determined by the experimenter or were based upon the objective evidence the individual had of his past performance in this field, e.g. reading and arithmetic tests in Sears's study. In the case of the anxious and the depressed patient, no outside group norm is imposed—it is rather an interiorized one, based upon the conception that the patient has of his abilities in relation to those of the group. Since he considers himself inferior, i.e. below the standard of the group, he behaves as if his performance had been compared with that of a superior group. In view of the dysthymic's poor opinion of his own ability, it is conceivable that he arranges the test situation in such a way as to obtain further confirmation of his inferiority, namely by placing his goal much above the level of his performance.

The hysteric, on the other hand, outwardly at least, appears to have a good opinion of himself. This is shown in his judgement discrepancy score, which was only very slightly negative and under increased motivation even positive. He tended to over- rather than underestimate his past performance. His work record is frequently poor and he shows little evidence of a sustained drive to get on. His goal-setting behaviour is in keeping with this. The hysteric sets a goal only slightly ahead of his performance, and if he fails to attain it at the subsequent trial, tends to adjust it readily in a downward direction.

Tests of level of aspiration of the type described above give a useful indication of the temperamental make-up of a person, a measure of self-confidence and of his estimate of his own abilities in relation to those of the group. Since the important part of the test consists in asking the subject to estimate the quality of his future and his past performance in a task with which he has been made familiar, these questions could be made part of any test that is repeatedly given.

In vocational guidance, tests of level of aspiration may provide an additional indicator of the candidate's ambitions in a given field. Some practical uses of level of aspiration tests could be suggested also in the educational sphere. For example, it should be possible to give children tests that have direct bearing upon their school subjects. A goal and judgement discrepancy score in one subject that differs significantly from those given by

the child in other subjects may serve as a pointer to the child's attitude to that particula subject and to the opinion that he has of himself in relation to the rest of the class Alternatively, if a number of tests show scores for a given child which deviate significantl from the class norm, such deviations may be indicative of the presence of neurotic trait and of difficulties in adjustment.

IV. Summary

1. Level of aspiration tests were given to 20 normal men and 33 normal women and t 100 male and 32 female neurotic service patients.

Estimates of future performance (goal discrepancy scores), estimates of past performanc (judgement discrepancy scores), responsiveness in the setting of goals to changes in th level of performance (index of flexibility), and the percentage of typical reactions t success and failure were calculated.

2. All four groups overestimated future performance and underestimated past per formance.

3. The behaviour of the women differed from that of the men in three aspects. The had (*a*) a significantly lower average goal discrepancy score, (*b*) a significantly lowe average judgement discrepancy score, and (*c*) a significantly higher index of flexibility.

This sex difference applied to the normal and the neurotic groups.

4. Significant differences were found in the responses of normal and neurotic subject The neurotic women had a higher positive goal discrepancy score than the normal women Normal subjects tended to underestimate their past performance more than neuroti subjects. The spread of scores of the neurotic groups was significantly larger than that o the normal groups. The distribution of the scores of the neurotics was bimodal.

5. A different patterning of the responses was obtained for neurotics in contrast t normals. The correlation between goal and judgement discrepancy was positive for th normal and negative for the neurotic groups. The correlation between judgement dis crepancy scores and index of typical reactions was negative for the normal and positiv for the neurotic groups.

6. A new code symbol substitution test was used to measure the level of aspiratio responses of twenty-nine male and twenty-nine female hysterical patients and of thirty-fou male and thirty-five female dysthymic patients. The hysterics were found to have a mor 'realistic' objective goal-setting behaviour than either the normal or the dysthymi subjects. The goal and judgement discrepancy scores of the hysterics were small and thei indices of flexibility high. The dysthymics showed wide deviations in their estimates o past and future performance from actual performance. Their indices of flexibility were low

7. Increased motivation was obtained by offering incentives. A comparative study o the behaviour of twenty male hysterical and of twenty-two male dysthymic patients unde these conditions was carried out. The increase in ego-involvement led to an accentuatio of the response pattern that had been found to be characteristic of these two reaction type

Since this paper was written, confirmatory evidence of the differences in level of aspiration behaviou has been obtained by comparing a further sample of normal men with the 50 hysterics and 50 dysthymi I had previously tested. Dr Emmanuel Miller, in a large-scale investigation, tested 178 normal men o the Triple Tester, using the technique described in this paper. He very kindly gave me permission t compare his data with that of the neurotic groups. When the curves were drawn for the goal and judgemer discrepancy scores of the three groups, both approximated normal distribution curves. The majority o the scores of the normal group was found in the central portion of the curves with the scores of th hysterics and those of the dysthymics at either end.

It gives me great pleasure to express my deep gratitude to Prof. A. Lewis for his guidance and continuous interest in this work. I am also greatly indebted to the Superintendent, Dr A. B. Stokes, for permission to use the clinical material.

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PSYCHOLOGICAL ASPECTS OF REHABILITATION IN CASES OF BRAIN INJURY[1]

By O. L. ZANGWILL[2]

I. *Introduction* (pp. 60–61). II. *The scope of rehabilitation* (pp. 61–63). III. *Principles of re-education* (pp. 63–67): (1) *Compensation* (pp. 63–64); (2) *Substitution* (pp. 64–65); (3) *Direct training* (pp. 65–67). IV. *Problems of resettlement* (pp. 67–68). V. *Conclusion* (p. 68). *References* (p. 69).

I. Introduction

The occupational adjustment of the disabled is an important province of that broad territory we nowadays know as social medicine. This province is as yet almost uncharted, but it is one in which the psychologist may reasonably hope to join forces with the physician and the resettlement officer in the common task of exploration. Indeed he is already doing so. Thus I may recall the conference arranged last year by the National Institute of Industrial Psychology at which physicians, psychologists and resettlement authorities debated their common problems. I may also call to mind a recent meeting of the British Psychological Society in London at which problems of resettlement were discussed. I therefore feel that no apology is needed for presenting a paper on rehabilitation to psychologists. The aspect of rehabilitation with which I shall be concerned, moreover, is one which by its very nature makes a special appeal to the psychologist. I refer to the rehabilitation of the brain-injured patient—a field in which I may claim some measure of personal experience based on five years' work with the Brain Injuries Unit in Edinburgh.

Cases of brain injury have a strong claim to be considered in a class of their own from the point of view of rehabilitation and occupational resettlement. I need not stress the peculiarly incapacitating nature of brain injury, nor the ways in which residual psychological disability may affect the brain-injured patient's capacity for work and social adjustment. These questions have been fully discussed in the more recent neurological literature and it is unnecessary to re-open them here.[3] I shall confine myself to a short discussion of the problems that confront the psychologist in brain injury work and the general principles which should govern his approach. Under war conditions, it is true, much of our work was necessarily empirical and any discussion of principles might well have been dismissed as academic. But the time has now come when we may, and I believe we should, take stock of our position, review our methods, and sketch the first outline of a systematic approach to the psychological problems of rehabilitation. I shall, therefore, attempt in this short paper, first, to consider the scope of rehabilitation in the field of brain injury; secondly, to review some general principles bearing on re-education; and thirdly, to call attention to some of the ways in which the psychologist can be of use

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[3] The reader is referred particularly to the 'Discussion on rehabilitation after injuries to the central nervous system' in the *Proceedings of the Royal Society of Medicine* for 1942. Especially relevant to the psychologist are the contributions to this discussion of Profs. Jefferson(4) and Cairns(1). Goldstein's book(3) on the after-effects of brain injuries in war may also be consulted for details of psychological symptoms and their management.

in the concrete problems of resettlement. It is hoped that the suggestions put forward in this paper may serve as a basis for discussion at present and as a guide to more systematic applied work in the future.

II. The scope of rehabilitation

"Although it is a new word, rehabilitation is an old purpose. All medical treatment has basically no other aim." These wise words of Prof. Jefferson (4) epitomize the modern tendency to eschew sharp distinctions between medical (or surgical) treatment on the one hand and rehabilitation on the other. Treatment in the traditional sense merges into rehabilitation, and rehabilitation must be regarded as an integral aspect of treatment. It is, as Jefferson insists, "...a continuum closely interwoven throughout with the general medical care of the patient" ((4), p. 296). It is important, further, to recognize that rehabilitation does not merely begin when the period of in-patient treatment is at an end. Measures of rehabilitation should start, in a graded way, as soon as the patient's condition permits. As Cairns succinctly puts it: "Rehabilitation begins when the patient begins to talk and respond properly" ((1), p. 300). The process should continue, with suitable upward grading, until the patient is thought fit to return to work and to take his place effectively in the community.

The general principles of rehabilitation which I have outlined were fully appreciated in the Centre at which I was privileged to work, and I was fortunate enough to gain access to the patients at a very early stage after their admission to hospital. Indeed, wherever possible, psychological testing and rehabilitation were begun at the bedside. We thus had a unique opportunity to study the patients through all the stages of their recovery and to adapt our methods of rehabilitation accordingly. Our problems, therefore, ranged from the psychological management of the acute case to the long-term re-education and eventual resettlement of the convalescent patient. At all stages we were fortunate enough to secure expert neurological and psychiatric advice and the willing co-operation of the auxiliary therapists.

Let us consider briefly the scope of rehabilitation at the early stages of recovery from brain injury. Here, of course, the brunt is necessarily borne by the medical and nursing staff. But there are often ways in which the psychologist can help in managing the acute case. Thus in the early stages the patient commonly displays a confusional state in which all his higher mental functions are in abeyance. His grasp is limited, his memory poor and his orientation fallacious. Memory for the accident, and for a variable period preceding it, is lacking. Emotional reactions are inadequate and ill-balanced. The various higher mental functions affected gradually recover of their own accord and nothing can be done directly to hasten their return. But it is sometimes possible to guide the patient in making the best use of his available faculties and in securing a good adjustment to the hospital setting. As orientation returns, for example, tactful explanation and reassurance are often of value in stabilizing it. Something may also be done to help the patient regain his memory for events which took place some time before the injury and which he has temporarily forgotten. Some degree of permanent retrograde amnesia remains, but the scope of the amnesia is, as a rule, much more extensive in the earlier stages. The shrinkage which it then undergoes can sometimes be hastened by reviewing with the patient the course of his life before the injury, or by helping him to reconstruct it on the basis of letters, diaries and information obtained from relatives. In much the same way, cases of

post-traumatic aphasia in process of regaining their speech can be helped, from the point of view of morale at least, by gentle reassurance and encouragement in conversation. From the point of view of occupational therapy, it is often possible to provide simple non-competitive games which attract the patient's interest and help to maintain his attention. Cairns (1) has advocated simple sorting procedures for this purpose, but I must admit that I found well-known pencil and paper games, such as naughts-and-crosses, even more suitable. The main contribution of the psychologist, in short, consists in an attempt to create the best possible conditions for the natural recovery of psychological function.

It is during the later stages of recovery that rehabilitation comes properly into its own, and the convalescent patient is the main concern of the psychologist. One is presented with a very wide range and variety of mental disabilities, all of which have to be most carefully studied and assessed if our measures of rehabilitation are to succeed. I cannot do better than quote the words of an acknowledged authority—Prof. Hugh Cairns—on the problems confronting us at this stage. "The central disturbance, the main organic cause of disability after head injury", writes Cairns, "is disturbance of mental capacity. It may occur at various levels of mental activity. At the lower levels there may be disturbance of speech, reading, calculating or orientation. Initiative, memory and concentration may be affected. At higher levels there may be impairment of judgement, or of the capacity for abstract reasoning. The patient cannot be satisfactorily guided through the stages of rehabilitation without an attempt on the part of the doctor—however halting it may still be—to assess his mind in terms of these functions" ((1), p. 300). These remarks of Prof. Cairns fittingly serve to define the psychologist's frame of reference. It falls above all to the psychologist to develop more adequate methods of assessing the various types and grades of disability to which Cairns draws attention. Much has already been done in this direction, but there is even more still to do. Nevertheless, there is increasing recognition on the part of the doctor that an assessment based, in part at least, on the outcome of psychological testing proves less halting and impressionistic than one based wholly on clinical findings.

Correct assessment is an essential pre-requisite for rehabilitation, but it is not rehabilitation itself. Rehabilitation, in its hospital stages at least, involves remedial exercises bearing on all the functions affected—mental and physical. It embraces occupational and speech therapy, physio-therapy, and related remedial disciplines. One very important aspect has come to be known as *re-education*, in so far as a direct attempt is made to retrain the patient in the various accomplishments and skills which he has lost as a result of his brain injury. The psychologist has important duties in this field. He is largely called upon to decide in any given case whether re-education at the psychological level is practicable, and if so, how it is best carried out. He has often to determine whether the approach should be by means of some direct method of training or whether it should proceed along an indirect route. Under the stress of war, as I have said, we were often obliged to answer such questions *ad hoc* with little reference to the underlying theory. As a result, I fear, we gained experience from our mistakes no less than from our successes. But in order to avoid faulty method and fruitless effort in the future, it is obviously desirable to inquire rather more closely into the nature of psychological changes in cases of brain injury. We wish to know, in particular, how far the brain-injured patient may be expected to compensate for his disabilities and the extent to which the injured human brain is capable of re-education. At the present state of our knowledge, alas, no categorical answers can be

given. But it is permissible to consider some very general principles bearing on these problems which may serve as terms of reference in the practice of re-education.

III. Principles of re-education

(1) *Compensation*

Let us consider first the concept of *compensation*[1] in relation to injuries of the central nervous system. By compensation is meant broadly *a reorganization of psychological function so as to minimize or circumvent a particular disability.* We are, of course, well acquainted with compensation as a biological reaction to injuries of other types. Thus everyone is familiar with the immediate readjustment of gait shown by an animal with an injured limb. In the case of brain injury, unfortunately, the position is made much more difficult by the complexity of cerebral function in general and by the tremendous specialization of the human brain in particular. In order to understand compensation, therefore, it is advisable to examine a little more closely the nature of the mental symptoms produced by cerebral lesions.

It has often been pointed out that the primary effect of a destructive brain lesion is to be regarded as a deficit state. This is apparent at the psychological, no less than at the neurological, level. Thus in organic disorders of visual perception, reaction to the various attributes of the visual field, such as form, colour, or spatial structure, may drop out collectively or in relative isolation. In disorders of motor skills, various purposive and constructive action-patterns are impaired or lost. At a higher level, linguistic and educational accomplishments over which the patient had faultless command are reduced or disorganized. Special abilities are often destroyed. But it is most important to recognize that every deficit state, as Hughlings Jackson long ago insisted, has its corresponding positive element. Indeed every positive manifestation of a destructive lesion represents the activity of healthy cerebral tissue. Now positive manifestations are of two types. The first comprises purely maladaptive symptoms which may be referred to the release of lower, normally inhibited, processes from higher cerebral control. Thus the choreic manifestations in cases with lesions of the motor areas or the jargon speech of the sensory aphasic are symptoms of this type. The second includes all those changes which can be treated in terms of an adaptive readjustment of the organism as a whole. These changes are compensatory. In cases of hemianopia, for example, the patient as a rule compensates very considerably for his restricted field of vision. It has even been claimed that such patients may develop a new centre of maximum sensitivity—the so-called pseudofovea—well within their functional visual field. An automatic readjustment of eye-movement regulation and control may also be observed. These changes serve to reduce very considerably the practical inconvenience of the restricted visual fields. They represent true compensation for disability.

Compensation takes place for the most part spontaneously and without the patient's explicit intention. In cases in which insight is good, however, a compensatory reaction evolved spontaneously can often be carried further by the patient's own efforts or by instruction and guidance on the part of the psychologist. Thus I have sometimes been in the position to help cases of severe motor aphasia, not by endeavouring to improve their speech directly, but by developing a compensatory method of expression. In many cases

[1] This term is used in its neurological and not in its economic sense.

writing proved to be the chosen channel and I provided the patient with a self-erasing slate on which to indicate his needs. In the same way, cases with recalcitrant disabilities of the right hand were regularly trained in the use of the left for skilled activities.

Therapy on the basis of compensation is, of course, a very obvious expedient and does not in itself warrant the high-sounding title of re-education. One is merely helping the patient to get round his disability along lines that he has already discovered for himself. But the effect of such guidance and help on the patient's morale is often considerable. As Cairns has written: "the value of encouragement by means of continued supervision cannot be overestimated" ((1), p. 302).

(2) *Substitution*

Substitution may be defined as *the building-up of a new method of response to replace one damaged irreparably by a cerebral lesion.* It is, of course, a form of compensation, but one in which the new method of response is developed by training very much further than the patient can achieve on his own initiative. It is, therefore, a method of re-education which has as its object the attaining of a given end by other than normal means.

Methods of substitution have been extensively developed in connexion with disabilities of other types. I need only mention lip-reading as taught to the deaf, and Braille in the case of the blind. The Braille method, as we know, makes reading possible by substituting tactual for visual discrimination and recognition. In brain-injury work, total blindness is, of course, rare, but complete inability to read, once called 'word-blindness', is relatively common. In this condition methods of substitution, somewhat analogous to Braille, may sometimes be applied with profit.

Perhaps I may be allowed to draw upon one example in my own experience to demonstrate the use of substitution in reading. I was concerned for some months in the systematic re-education of a patient who had sustained a severe injury to the posterior part of his left cerebral hemisphere. This man displayed, in addition to some aphasia, a complete inability to recognize written or printed symbols. But it was soon discovered that the patient, if allowed to trace outlines of letters with his forefinger, could readily identify them. Having traced the letters of a word in this way the patient was generally able to pronounce it, or at least to grasp its meaning. This ability was used as the basis of re-education. After progressive practice for several weeks the patient became able to read coherent material. In the course of this period, the tracing of letter outlines was replaced by continuous, rapid, writing movements which the patient executed with his finger tip on the table or on his knee. Eventually even these responses dropped out except in the case of words of unusual difficulty. I may add that, after three or four months of training along these lines, the patient became able to read material, both written and printed, of average difficulty and at a speed which, though much below the normal, sufficed for practical purposes. This patient was able to return to his old job as an electrical engineer.

We see in the case I have mentioned the building-up of a substitute method of reading based on tactual and kinaesthetic patterns which effectively circumvented the visual agnosia. But this case was in many ways an exceptionally favourable one for re-education, and it must not be supposed that equally good results are given by all such patients. In many, however, even in those who remain gravely disabled, it is often possible to develop some degree of substitutive replacement. Thus I recall a case of motor aphasia with a severe disturbance of calculation. It was hoped to rehabilitate this patient sufficiently to

enable her to go back to simple household duties. One of these duties, by no means unimportant, was the management of money and the checking of household bills. Investigation showed that the patient could add correctly and appreciated the relative value of pounds, shillings and pence. But she was quite unable to divide. Her difficulty here was in part overcome by the following procedure. On a simple money sum, the patient was told to add the pence in the usual way. This she could do correctly. She was then instructed to make a number of dots on the paper to represent the total pence, to put a ring round every group of twelve, and to count the remainder. She then counted the number of ringed groups and added this total to the shillings. A similar procedure was adopted here except, of course, that groups of twenty were made instead of twelve. After three weeks' practice the patient developed considerable facility in this round-about method of addition. I may add that her morale and self-confidence were thereby much improved.

The amount of substitution possible in any given case depends on a variety of factors, among which the age of the patient and the extent and locus of the lesion are especially important. In general, more substitution is to be expected in children and young adults than in the elderly, and in those with circumscribed rather than diffuse cerebral lesions. But much depends, too, on the personality of the patient and the nature and severity of the mental symptoms. Curiously enough, compensation is often more apparent in a severely disabled patient than in one less seriously handicapped. As Goldstein (2) has pointed out, an organism tends to retain an old and well-tried method of expression if it performs to some extent at least its proper function. But if this is no longer possible readjustment takes place and compensatory behaviour is observed. It is evident, therefore, that no hard and fast rules can be laid down as to the indications for substitutive training. But if the patient tends to compensate his disability in a particular direction it is usually profitable to develop it further by training.

(3) *Direct training*

We have been dealing so far with disabilities which do not genuinely recover. It is now time to ask whether direct, as opposed to indirect, methods of training are possible, and if so, what results may reasonably be expected of them. In the earlier ventures in re-education, much hope was pinned on what has been called the principle of *vicarious function*. Thus it was supposed that improvement with re-education was due to the taking over of the functions of an injured part of the brain by some other part that had escaped injury. In cases of unilateral lesion, especially, the intact hemisphere has been held capable of acquiring, to some extent at least, the functions of the injured one. Unfortunately, however, the existence of vicarious function is exceedingly difficult either to prove or disprove. Although there is some evidence of vicarious function in animals, it appears doubtful whether the functions reacquired are, in fact, carried out vicariously by any one particular part of the brain (Lashley (5,6)). In man, furthermore, the position is even more uncertain and confusing. In the first place, the fact that a cerebral injury may provoke a *temporary* loss of function of other parts of the brain, themselves uninjured, often makes it difficult to distinguish slow spontaneous recovery of function from recovery based on re-education (von Monakow (7)). And, in the second place, it is likely that the most specialized mental functions in man, such as language, depend on relatively well-circumscribed areas of the brain, and that the function of these areas, when damaged, cannot be taken over directly by other areas. Thus it has been suggested that improvement

with retraining in aphasia depends, not on the formation and vicarious function of a new 'speech centre' in the right hemisphere, but upon the fact that the areas normally concerned with language have not been wholly destroyed. The parts left intact take over the functions of the system as a whole (cf. Goldstein (2) and Lashley (6)). Even if we abandon the concept of localized vicarious function, therefore, we need not abandon wholly the possibility of direct re-education. Indeed I have seen many cases of brain injury which appear to benefit appreciably from direct approach of this kind. Some of these cases may be briefly considered.

In the first place, my own experience suggests that certain types of executive disability respond favourably to direct training. Many of these fall within the province of the physiotherapist, and I am not competent to discuss them in detail. But I have seen a number of cases with disorders of articulation due to cerebral injuries in which I am satisfied that speech-therapy of the orthodox type was of great value. A patient can be taught to pronounce and combine essential speech sounds which he has entirely lost. This cannot very well be regarded as substitutive learning, since the patient acquires the very co-ordinations which he has lost. It is true re-education. Whether it depends on the plasticity of some part of the motor speech area that has escaped injury, or upon the vicarious function of some other part of the brain, I cannot say. But I see no reason to doubt the value of conscientious speech training in dysarthric conditions.

In the second place, I am satisfied that some cases with intellectual handicaps benefit appreciably from simple exercises and training of an ordinary scholastic kind. Controlled studies are difficult to carry out, but Weisenburg & McBride's work (9) suggests that exercises of this type appreciably hasten the natural course of recovery in aphasic disorders. In some such cases, I believe, we are carrying out true re-education, but in others improvement may well be due to the gradual overcoming of the effects of shock—or *diaschisis*, as von Monakow (7) called it. Thus I have noticed that many patients with a language disturbance make errors in simple multiplication. These errors rapidly drop out if the patient is required to revise or relearn the multiplication tables. But it is seldom found that the patient has to start from scratch, in the manner of the young child, and I cannot believe that he is really acquiring a new serial habit. It seems much more plausible to suppose that the original habit-system undergoes a temporary and reversible deterioration. It shows a certain 'functional inertia' due to diaschisis. One may suppose further that practice helps to overcome this inertia by re-exciting the system which forms the cerebral basis of the habit. I admit that this interpretation is speculative and that I have no real evidence. But whatever the theory we prefer, I am satisfied that systematic exercises and progressive practice can play a real part in re-education. In this connexion, too, I may recall the old observation that in retraining cases of aphasia a stage is very often reached at which the patient is found to use not only words which he has been taught but other words too. This may likewise reflect a sudden improvement in cerebral function mediated by training.[1]

I think it is fair to conclude that direct, as opposed to substitutive, training has a real, though limited, part to play in re-education. It is indicated especially in cases with

[1] Lashley calls attention to this phenomenon in his important discussion (6) of the factors limiting recovery after central nervous lesions in animals and man. But he rejects the concept of diaschisis as an explanatory principle. He supposes that the effects commonly attributed to diaschisis may be explained more plausibly in terms of the restitution of some essential mechanism deranged by the lesion. As this mechanism is restored, either by recovery or training, its dependent functions automatically reappear.

executive disabilities such as motor aphasia. In the form of progressive practice, moreover, it may contribute, perhaps by overcoming inertia due to shock, to the rate of recovery of psychological function in general.

IV. Problems of resettlement

I wish, in conclusion, to review very briefly the scope of the psychologist's contribution to problems of *resettlement* and to make some suggestions as to how this contribution could be increased. The most effective contribution, as I have said, lies in the proper assessment of residual disability. This involves determining not only what the patient has lost, but also his probable capacity to respond to training in the future. As Cairns has pointed out, "After severe head injury the question will arise whether the patient is likely to be fit for his previous work, and this will often largely depend on his capacity to learn" ((1), p. 300). I have stressed elsewhere the value of psychological tests of learning in this connexion (10,11), and they would appear to be well worth developing more systematically. When used in conjunction with progress-records in the occupational therapy department, they have real value in predicting a patient's response to vocational training.

In view of the present vogue of psychometric techniques, it is perhaps necessary to insist that disability at the psychological level cannot be measured in any simple way and expressed in mathematical terms. Quantitative tests of deterioration are valuable but never sufficient. Not only test results, but experience and even—it must be confessed—guesswork, are bound in some degree to colour our judgement. But I think it is fair to conclude that modern psychological test techniques, when used with due caution, provide a useful supplement to the data normally available for assessment in regard to resettlement. This is a field that could well be developed in the future.

The assessment of patients with a view to proper resettlement has two aspects that are not always clearly distinguished. These aspects may be termed negative and positive. The *negative* aspect represents the range of possible occupations from which the patient is *debarred* on account of his disabilities. The *positive* aspect represents the range of possible occupations which may, from the medical point of view, be considered as still open to him. The negative aspect places the patient's disabilities in the foreground; the positive, his intact capacities along with his original endowment and previous training. Let us consider a hypothetical case of residual dysphasia due to brain injury. On the negative side, the language defect must be held to exclude all but the simplest clerical work. On the positive side, however, good intelligence together with unimpaired learning would render the patient exceptionally well-suited to take part in a vocational retraining scheme, perhaps with a view to eventual resettlement in a skilled industrial job. It will be seen, therefore, that assessment of disability forms only one part of the psychologist's task. He must also assess all those factors which, on the positive side, may have bearing on retraining and resettlement.

This leads me on to a final point, namely, that the psychologist concerned in rehabilitation work should have some knowledge of the industrial aspects of his subject. At the recent conference on the occupational readjustment of the disabled organized by the National Institute of Industrial Psychology, a speaker representing the Ministry of Labour objected to the present form of medical report on the ground that " . . .it invites from the doctor an expression of opinion upon suitable employment, a responsibility which properly

belongs to the industrial not the medical expert" (8), p. 5). The same speaker went on to observe that: "Medical men cannot be expected to know the physical requirements of the many thousands of different occupations unless they have specialized in the work" (loc. cit.). It is plain that in considering resettlement from the psychological point of view it is necessary to bear in mind the economic and industrial, in addition to the clinical, factors which bear on the patient's choice of occupation. In my own work, I fear, I found myself sharing the alleged ignorance of the medical man regarding industrial requirements much more frequently than the omniscience of the industrial expert. Yet I feel sure that some knowledge of industrial psychology would have fitted me to play a useful liaison role between the clinical staff of the Unit in which I worked and the Ministry of Labour Resettlement Officers. Perhaps in the future some training in industrial psychology will be regarded as an indispensable qualification of the clinical psychologist. His value in relation to problems of resettlement would thereby be much enhanced.

It must be borne in mind that not every case of brain injury can be satisfactorily resettled in competitive economic life. Many require a far greater degree of protection and supervision than it is possible to exercise under ordinary working conditions. If, as is more than possible, we see the development of special centres at which these patients can live and work for long periods, it is likely that a number of problems will arise in the general field of occupational psychology. This suggests a further field of development in which psychologists may well have their part to play.

V. Conclusion

I have endeavoured to review the problems that confront the psychologist in brain-injury work and to consider the part which he is called upon to play in rehabilitation and resettlement. I have also tried to define, in a tentative manner, some general principles which may serve as a frame of reference in the practice of re-education. If at times I have strayed from the strict path of practical application into the tortuous maze of cerebral physiology, it is because I believe that the rationale of re-education is to be sought in the study of the brain and the mechanisms whereby it compensates for injury to its parts. The more we find out about the brain and its functions the more likely are we to discover methods of rehabilitation based on strictly scientific principles. In this quest, no less than in the practical routine of resettlement, the methods of experimental psychology have the chance to prove their worth.

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AN ATTEMPT TO TEST HIGH-GRADE INTELLIGENCE[1]

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I. *Introduction* (pp. 70–72). II. *Design of Test AH* 5 (pp. 72–74). III. *Aims and method of enquiry* (pp. 74–75). IV. *Results* (pp. 75–80): (1) *Correlation between tests and parts of tests* (pp. 75–77); (2) *Speed and accuracy* (pp. 77–78); (3) *The part played by bias* (pp. 78–79); (4) *Differences between university faculties* (pp. 79–80). V. *Summary and conclusions* (p. 80). *Appendix* (p. 81).

I. Introduction

The main aim of the research discussed was to determine whether it is possible, and meaningful, to discriminate between highly intelligent individuals, on grounds of their general intelligence. It has been fairly well established with *unselected* groups that intelligence is one of the most important single factors to take into account when, for instance, an individual is being assessed for vocational or clinical purposes. It seems possible, however, that within a highly selected superior group differences in intelligence play a relatively small part as compared with differences in 'flair' or specific aptitude, in interest, and in temperament and character. Previous work on the intelligence of high-grade[2] groups has suggested that the importance of intelligence as assessed by psychological tests decreases as the mental stature of the subjects increases: intelligence test scores have tended to show only a slight relationship with independent criteria of proficiency. However, apart from the common inadequacy of these criteria, it seems doubtful that many reliable data have been obtained, since the tests used in this connection have rarely, if ever, fulfilled the conditions essential for a high-grade intelligence test.

They have generally failed on one or more of the following counts:

(*a*) They may bunch the intelligent subjects closely at the top end of the scale. In the Progressive Matrices Test, for example, which has been widely used with excellent results, the top 10% of an unselected adult group score between 54 and 60, inclusive.[3] This is clearly rather a narrow range in which to crowd 10% of the population. When, however, this range is contrasted with that obtained by the bottom 10% of the same unselected group, 0–27,[3] the unsatisfactoriness of the test for use with preselected subjects of high intelligence is obvious. These figures imply that the difference between, for instance, 55 and 58 marks on Progressive Matrices claims respect, although the difference between say, 15 and 18 would be treated as of little consequence. It is well known that differences between test scores within the middle 20 or 30% may have less significance than the same differences between scores towards the extremes. But when the tailing off of the curve is markedly unsymmetrical and bunching occurs at the top end, the test can scarcely be as useful for high-grade as for low-grade subjects.

(*b*) The test may yield a wide range with a good distribution from a group of high-grade subjects, but still fail to discriminate between them on *intelligence*. This seemed to happen

[1] Based upon a paper read to the Cambridge Psychological Society in May 1946.

[2] In this paper 'high grade' signifies, roughly, the top 10% of the population in respect of intelligence.

[3] Quoted by Edholm and Gibson in *The Lancet*, 26 August 1944, p. 294.

with Test AH 4 (devised by the writer), when applied to superior groups such as undergraduates or research workers.

The maximum number of marks obtainable on AH 4 is 130. With a group of 690 factory operatives, this test gave a range of 0–96, a mean of 36, and a symmetrical, uni-modal distribution. A group of 120 Cambridge students gave a range of 62–126, with a mean of 96; the distribution curve was only slightly less symmetrical. (The AH 4 scores obtained from some 150 research workers and lecturers gave a very similar picture to that of the students, with the range slightly extended at both ends.) With a range of 64, such as the students gave, and a reasonably normal looking distribution, AH 4 is evidently not open to the particular objections discussed under (*a*) above. However, inspection of the answer sheets suggested that the discrimination between these highly selected subjects was on grounds not so much of intelligence as of *speed*.

Almost every subject gave the right answer to a very large proportion of the questions he attempted. Thus the test score of a high-grade subject—when compared with the scores of other high-grade subjects—is apparently a function of the number of questions he attempts rather than of the accuracy with which he answers the questions. A subject, *X*, who attempted 85 questions, would get perhaps 82 right; and another, *Y*, who attempted 125 questions, would get perhaps 120 right. Both these scores would place *X* and *Y* in the top grade, as compared with the population generally, but it looks as though there is only one respect in which we can be certain that *Y* is superior to *X*, namely on speed.

This problem apparently does not arise when the test is applied to the unselected cross-section of the population for whom it was originally designed. Inspection of the answer sheets of unselected subjects reveals a reassuring number of errors.

(*c*) The third reason for failure in tests of high-grade intelligence is concerned with the actual structure of the tests.

First, many so-called 'tests of general intelligence' have an overwhelmingly verbal bias. They contain far more questions requiring the manipulation of words for their solution than questions requiring the manipulation of diagrams or numbers. This may put the non-verbal thinker at a disadvantage and may tend also to stress the factors of education and knowledge—undesirable in a test purporting to assess potential capacity.

Secondly, in many tests, as the questions increase in difficulty, so the dubiousness of the 'correct' answer increases. Synonym and Antonym questions may be included, the alleged solutions of which are controversial. Analogies may be used in which no defined relationship is discernible between the first two terms, even in the light of the third term; and classification problems may be devised which leave the subject doubtful about which of a number of possible group characters he is expected to use. Similar objections apply also to questions without a verbal bias which are used in some of the existing tests designed for superior adults.

It is clearly more difficult to devise questions each with only one defensible answer for high-grade people than for children or for average and mixed groups. But, equally clearly, the integrity of the questions is still more vital for the intelligent subject, since he will tend to be more highly critical of slipshod thinking, more apt to recognise and worry over ambiguities and subtle shades of meaning.

(*d*) The fourth objection is to the type of intelligence test which increases its difficulty by making increasing demands on the knowledge and education of the subject. For

example, towards the end of the Sames and Opposites part of the National Institute of Industrial Psychology's Group Test 33 are found several rather unusual four- and even five-syllable words. Familiarity with such words may bear some relation to innate intelligence but at best it must be a somewhat indirect one.

II. Design of Test AH 5

This section is concerned with the means used to overcome the above objections to tests of high-grade intelligence. Since bias cannot be wholly eliminated the three most important types, the verbal, the arithmetical and the visual, were all introduced.[1] As far as possible the testing of specific aptitudes was avoided and the part played by education minimized. Care was taken to see that there was one, and only one, correct answer to each question—this being included in the five given answers, when the subject was required to choose one from these. Problems were devised which, it was hoped, would demand genuine thinking from the intelligent subject.

As problems increase in complexity, there is perhaps a tendency for their solutions to become less clear-cut, to become arguable matters of opinion rather than rigid matters of fact. It is not quite clear how far this vagueness is essential in the consideration of certain types of problem, and how far it merely implies that muddled or insufficient thought has been devoted to the matter. In any case, questions of this kind were rejected and only those selected which have one objectively correct answer. Although it was realized that this might curtail the scope of the question paper it was considered that the gain would be greater than the loss. By these means it was hoped that the criticisms concerning structure of test, education and speed would be met and that the criticism of 'bunching' would be found not to apply; that AH 5, when given to a group of intelligent subjects, would yield a reasonable range of score, and a sufficiently normal distribution to render the usual statistical methods permissible.

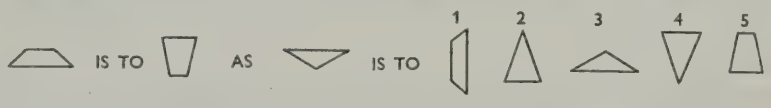
The same form was used for AH 5 as for AH 4; there were two parts of equal length, the first having a verbal and arithmetical bias, the second having a visual bias. In order to minimize the importance of the speed factor, the number of questions in each part was reduced and the time limit was raised from 10 to 20 min. for each part. With questions of the standard chosen, the new time limit was still too short for any but the quickest subjects to finish: this was essential if the crude scores were to be comparable. Subjects were instructed to take the questions in any order, but in fact the vast majority worked straight through the test, with an occasional omission.

Although Part I has a verbal and arithmetical bias and Part II a visual bias, AH 5 is not a test of verbal or numerical ability or of spatial perception. The high standard in Part I is not achieved by using long and obscure words or by trespassing in the field of advanced mathematics; similarly in Part II, increasing difficulty is not achieved by increasing the complexity of the diagrams.

The necessity for clear and careful thought in the test is obtained mainly in four ways:

(*a*) The relationship between the words, figures or diagrams may be twofold; for instance:

(i)



[1] 'Visual' here signifies reasoning in terms of relations between seen shapes.

In order that the subject should recognize Answer 2 as correct, he must observe that the original figure has been both elongated and inverted. It will be seen, however, that the diagrams are, in themselves, very simple.

This two-fold relationship has been used also in some of the verbal questions in Part I of AH 5—those which replace the straight Synonym-Antonym type of question. For instance:

(ii) Which one of the five words on the right bears a similar relation to each of the two words on the left?

		1	2	3	4	5
Teach.	Retinue.	Learn.	Following.	Train.	Instruction.	Retain.

(iii) Which one of the five words on the right bears a similar relation to each of the two words on the left?

		1	2	3	4	5
Thin.	Clear.	Fat.	Narrow.	Fog.	Thick.	Dull.

It may be seen that the subject has to grasp the principle of 'similar relationship'; to determine whether this relationship is one of similarity or opposition; and to carry in his head the meanings of both the words on the left. If he takes these words one at a time, he will have difficulty in understanding that 'train', in example (ii), and 'thick', in example (iii), form a kind of link between the respective pairs of words on the left.

(*b*) The subject may need to apprehend a second order relationship. For instance in

(iv) "3, 5, 7, 9, 17, 33, ...",

the existence of a series *within* the series (2, 4, 8, 16,...) must be recognized to get the answer.

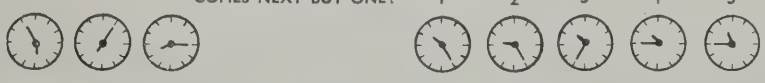
(*c*) The difficulty may depend on the unexpectedness of the question, on some juxtaposition of ideas not usually found in the same context. For instance:

(v) School is to porpoises as crocodile is to...

1	2	3	4	5
Alligators.	Tears.	Schoolgirls.	Dolphins.	Teachers.

(*d*) Grasping the principle correctly is necessary but not always sufficient. Sometimes the subject is required to go yet a step further; for instance, in the time series indicated in (vi):

(vi) WHICH OF THE FOLLOWING COMES NEXT BUT ONE? 1 2 3 4 5



Most subjects will realize that there is a difference of 70 mins. between the first and second clock, and again between the second and third. A good many, however, will choose the clock that comes *next* (Answer 2) and some will choose the one that comes next but *two* (Answer 5).

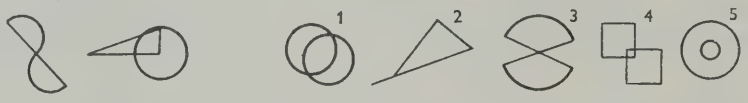
Sometimes this form of question requires more thought as well as greater care. For instance, in

(vii) "90, 72, 54, 36, ... Give the next but two members of the series,"

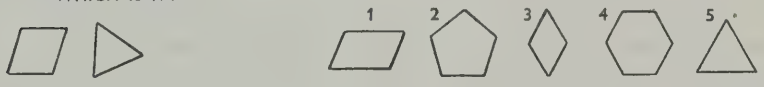
in order to arrive at the correct answer (-18), the subject has to recognize the principle on which the series is built (9×10, 9×8, 9×6, etc.); to bear in mind that he is asked to skip the next two members of the series; and, therefore, to realize that he must cross over from positive to negative numbers. There is a strong tendency to give the answer '0' to this kind of question, for one reason or another.

In order to avoid using questions with no really satisfactory answer, or with more tha one, certain current types of questions were adapted to a less ambiguous form. In Part I for instance, Classification questions occur of the form "Which of these five figures lack a quality possessed by the other four?" or "...possesses a quality which the other fou lack?" But in addition to the five figures on the right, two are shown on the left, bot of which possess the quality in question. For instance,

(viii) THE TWO FIGURES ON THE LEFT HAVE A FEATURE IN COMMON. ONE, AND ONE ONLY, OF THE FIGURES ON THE RIGHT *HAS* THIS FEATURE. WHICH IS IT?



(ix) THE TWO FIGURES ON THE LEFT HAVE A FEATURE IN COMMON. ONE, AND ONE ONLY, OF THE FIGURES ON THE RIGHT *LACKS* THIS FEATURE. WHICH IS IT?



Answer 3 in Example (viii), and Answer 1 in Example (ix) have, and lack, respectivel the qualities possessed by the appropriate pairs of figures on the left. The inclusion *both* types of question—the 'possessing' and the 'lacking' of the requisite quality—ensur that the subject who does well will have remained mentally alert throughout the test.

The above examples illustrate the form of some of the questions comprising AH None of the questions reproduced is used in the actual test.

The types of question in Test AH 5 are as varied as is consistent with giving an exampl at the beginning of the test, of each of the principles used in the test proper. If very mar different principles were used, completion of the preliminary examples would be liable take as long as the test itself. At the same time, a general intelligence test which uses on one, or a very few types of principle is unlikely to do justice to all subjects. It seems a goo working rule to use as much variety in the test material as may be practically possibl

III. Aims and method of enquiry

In the Michaelmas Term, 1945, some 180 Cambridge University students took Tests AH and AH 4. Almost every university faculty was represented, the group being as random sample of students as is consistent with the volunteer method of obtaining subjects. The were about six times as many men as women. They took AH 4 first, with its usual tir limit of 10 min. for each Part, and then, changing from an ordinary pencil to a blue or they finished the test in their own time. They were encouraged to check over any questio and, in the event of their changing their minds about an answer, they were asked to lea the original pencilled answer and write the new one, in blue, at the side. The extra tir taken over Parts I and II respectively was recorded. At their next sitting, they took AH Part I, in the same way—first with and then without a time limit; and finally, the thi time, Part II of AH 5.

The objects of adopting the above procedure were as follows:

(*a*) To correlate AH 4 with AH 5. If this correlation proved fairly low, there would some evidence that the two tests, though similar in type, were testing something differe

(*b*) To compare the correlation of AH 4 Limited and Unlimited time with the correlation of AH 5 Limited and Unlimited time. If the assumption is right that AH 4 Limited tests mainly speed, when applied to a preselected, highly intelligent group, the correlation between AH 4 scores on Limited and Unlimited time should be low. On the other hand, if AH 5 does successfully test intelligence at a high level, and minimizes the speed factor, a far higher correlation between AH 5 scores Limited and Unlimited should be found.

(*c*) To compare the scores (Unlimited) on both tests with the actual time taken. This would, it was thought, be of intrinsic interest, throwing light perhaps on the general question of the relationship between speed and accuracy in thinking. Would the subjects who took the longest time, when working at their leisure, prove to be *more* or *less* accurate than those who work naturally fast? Or would generalizing on the relation between speed and accuracy in this connexion prove injudicious?

Apart from this it appeared that any difference between the correlations of the two Unlimited test scores and time might provide further evidence on the existence and extent of differences between the qualities measured by the two tests.

These three were 'priority' aims. It was hoped also:

(*d*) To compare the correlations of scores on the two separate Parts of the tests, with themselves and one another, with and without a time limit, in order to see how the relationship between Part I and Part II is affected by (i) cutting out the time factor, (ii) increasing the difficulty of the questions.

(*e*) To analyse the scores on both tests according to university faculty and sex.

IV. Results

It was possible to use the results of only 165 of the original 180 subjects. The remaining fifteen were omitted either because they produced an incomplete set of test scores—having missed one of the sessions or failed to record their time—or because English was not their native tongue. Since Part I of both tests has a verbal bias it was thought that inclusion of these latter subjects might have an incalculable effect on the test scores. The following discussion, then, is on the results of 165 subjects. For the frequency distribution curves of the two tests, see Appendix (p. 81).

The consistency of the test was established on ninety-four students, who were retested early in the Summer Term, some five months after their first testing. (The remaining seventy-one did not volunteer for the second time). Product moment correlations between the two testings gave the following values (on time Limited): total score with total score, $r = 0{\cdot}84$; Part I with Part I, $r = 0{\cdot}7$; Part II with Part II, $r = 0{\cdot}81$.

The test had been slightly altered after the first testing, in the light of the analysis of the answers and of certain criticisms made by the subjects. Several questions in each Part were altered and the order of questions was improved. It is likely, therefore, that a test-retest on the new AH 5 would show slightly higher consistency.

(1) *Correlations between tests and parts of tests*

Table 1 gives the correlations on Parts and Total Scores, of Tests AH 5 and AH 4.

The correlation of 0·712 between the totals of AH 5 Limited and AH 4 Limited was, from the writer's point of view, disappointingly high. It is true that the relationship was closer on Total score than on either of the separate Parts: AH 5 Limited II correlated with AH 4 Limited II, 0·697; whilst AH 5 Limited I correlated with AH 4 Limited I only

Table 1

		AH 4 Limited			AH 4 Unlimited			AH 5 Limited			AH 5 Unlimited		
		Pt. I	Pt. II	Total	Pt. I	Pt. II	Total	Pt. I	Pt. II	Total	Pt. I	Pt. II	Total
AH 4 Limited	Pt. I		0·606		0·344	0·233		0·548	0·428		0·403	0·239	
	Pt. II	0·606			0·227	0·462		0·472	0·697		0·394	0·469	
	Total						0·411			0·712			0·496
Unlimited	Pt. I	0·344	0·227			0·515		0·434	0·333		0·519	0·419	
	Pt. II	0·233	0·462		0·515			0·383	0·503		0·537	0·615	
	Total			0·411						0·569			0·688
AH 5 Limited	Pt. I	0·548	0·472		0·434	0·383			0·472		0·762	0·382	
	Pt. II	0·428	0·697		0·333	0·503		0·472			0·428	0·726	
	Total			0·712			0·569						0·763
Unlimited	Pt. I	0·403	0·394		0·519	0·537		0·762	0·428			0·544	
	Pt. II	0·239	0·469		0·419	0·615		0·382	0·726		0·544		
	Total			0·496			0·688			0·763			

Coefficients greater than 0·155 are significant at the 5% level.

to the extent of 0·548. The correlation of 0·712 between the Totals suggested that AH 5 test had little new to offer. However, a comparison of the Limited and Unlimited scores on the two tests appeared to contradict this.

The correlation of AH 4 Limited with AH 4 Unlimited gave as low a value as 0·411. This was in line with the hypothesis that AH 4 when applied to a high-grade group discriminates on speed rather than intelligence. If a speed test is given to a group, first with a time limit and then (to the same subjects) with instructions to work at leisure, the two sets of scores would scarcely be expected to come in the same order. AH 5 (Total) Limited correlated with AH 5 (Total) Unlimited, however, gave a value of 0·763—much higher than the corresponding AH 4 relation and slightly higher than the relation between AH 4 and AH 5. This difference between the tests appears to run right through: the same order of difference is found when comparing time Limited and Unlimited on the two Parts separately. For *Part I's*, AH 5, $r = 0{\cdot}762$; AH 4, $r = 0{\cdot}344$. For *Part II's*, AH 5, $r = 0{\cdot}726$; AH 4, $r = 0{\cdot}462$. In each case the difference between the coefficients for Limited and Unlimited in the two Tests is significant at the 5% level.

This provides some evidence that AH 5: (*a*) relegates speed to a comparatively minor role, (*b*) may effect some discrimination between high-grade subjects, on grounds of *intelligence.*

(2) *Speed and accuracy*

Inspection of the two distributions again supports this view: the correlation scatter diagram of AH 4 Limited against Unlimited shows that a substantial minority of the subjects in the bottom third on AH 4 Limited achieve the top third on AH 4 Unlimited. These will presumably be the slow but sure thinkers who tend to be under-rated when compared, on the basis of an ordinary intelligence test, with their intellectual equals. The diagram discloses also that about a quarter of the people who do well on AH 4 Limited fail to get into the top half on Unlimited.

On the other hand, in AH 5, the association between Limited and Unlimited scores appears to run right through. None of the people who do badly on AH 5 Limited do well on AH 5 Unlimited; and practically all the subjects who do *well* on AH 5 Limited do well also on AH 5 Unlimited. This suggests that the slow, sound, careful thinkers are perhaps coming into their own on AH 5—the subjects, for instance, who insist on checking their answers on AH 4 and achieve, perhaps, only 70 marks. For the intelligent subject to check his answers on *AH 4* is a waste of time: it will almost certainly reduce his score. But the intelligent subject who checks his *AH 5* answers will very likely benefit from it.

However, these 'slow, sound, careful thinkers' are perhaps rarer than is commonly supposed. Examination of the actual times taken revealed a tendency for the subjects who prefer to work fast to do better than the subjects who work more slowly for choice. The actual times taken on the two tests were compared with the subjects' respective scores in Unlimited time.[1]

The range of times taken on AH 4 was 20–56 min., with a mean of 31·8. These times include the set time limit of 20 min. on AH 4. Thus the few subjects whose Unlimited time was 20 min. did actually finish the test within the time limit and did not wish to

[1] 'Unlimited' is probably not quite accurate, as one or two subjects had a supervision after the test; others, a team practice or a tea party; and others clearly wanted to leave with a friend—who had worked through the test rather quickly. However, these cases were surprisingly rare, as far as the testers could judge. Most people stuck doggedly to their papers until they had produced a satisfying answer to each question.

check any of their answers. The range of times on AH 5 was 40–130 min., set time again included. The mean was 76·2.

The times taken on the two tests correlated 0·588. This agreement was due more to the quick subjects being consistent in their speed than to any marked consistency among the slow ones (apart from one subject who was extraordinarily slow on both tests). A few of the slow AH 4 subjects were reasonably fast on AH 5 and some of the slow AH 5 subjects were up in the quick half, on AH 4.

On both tests, score Unlimited correlated negatively with time taken; i.e. on both tests, the longer the time the smaller the score. The correction was $-0{\cdot}168$ for AH 4 and $-0{\cdot}161$ for AH 5. Both these values are just significant.

These results suggest that the people who work naturally fast tend also to be mentally superior to the slower thinkers. However, the size of the correlation coefficients—no less than experience with people in everyday life—suggests that the exceptions to this are numerous. In fact, many subjects with a high score on the tests were below average on speed, and quite a few of the quick ones *scored* lower than average.

(3) *The part played by bias*

The next point to investigate was the relation between Parts I and II of the tests. The degree of correlation to expect between Part I and Part II of AH 4, with different groups, had already been established. It was desirable to learn how this relationship would be affected (*a*) by removing the time limit and (*b*) by using a test of similar construction but of a much higher standard.

The correlation between Parts I and II of AH 4 Limited with the group under discussion was 0·606. This is about the usual correlation with such a selected group. More mixed groups give a correlation round about 0·75, but with university subjects the correlation between the two parts has always been of the order of 0·6. On Unlimited time, the correlation between AH 4 Parts I and II was slightly lower, 0·515.

On AH 5 Limited, the correlation between the two Parts was lower still, 0·472; and on AH 5 Unlimited, 0·544. The biggest difference is between AH 4 Limited and AH 5 Limited, the correlations equalling 0·606 and 0·472 respectively. This difference is not quite significant, but it suggests, perhaps, that at the higher levels differences in bias play an increasingly important part.

This difference is not likely to be explicable in terms of the actual test—that a greater difference in bias had inadvertently been introduced between the Parts of AH 5 than of AH 4—since on Unlimited time, *AH 5* I and II correlated slightly *more* highly (0·544) than did *AH 4* I and II (0·515). It looks as though the difference may be a genuine psychological one—as though when subjects are confronted with a problem that 'extends' them fairly fully, their possession or lack of any particular bias has more effect than when they are required to solve some relatively easy problem.

At first sight it appears odd that the two correlations on Unlimited time should fall in between the two on Limited time. It may be, however, that preferential ability or interest carries less weight when the subject is not pressed for time.

On this question of bias, it is perhaps worth pointing out that Part II correlated consistently more highly with Part II than did Part I with Part I—whether the comparison was made as between the tests or as between time Limited and Unlimited. In each of the four pairs, Part II proved to be more closely associated with the other Part II than was

Part I with *its* counterpart. This may of course be explicable by the fact that Part II contains *one* main bias (a visual one), whereas Part I contains *two* main biases (verbal and arithmetical).

(4) *Differences between university faculties*

The results on university faculty and sex are inconclusive owing to the inadequacy of the data. These will, therefore, be dealt with very briefly.

Each faculty that had twelve or more representatives was considered separately. History and Classics, however, were grouped together since there were only seven students of each, and the faculties have a good deal in common. There was also a 'miscellaneous' class composed of seventeen agriculturalists, economists, geographers and psychologists. This brought the total number of faculty groups up to nine.

The mean *AH 4* Total score for each faculty, or group, ranged from 105·9 for Mathematics to 82·9 for English. Engineering and Mechanical Sciences came high; Modern Languages and Miscellaneous, low. On *AH 5* Total, the means ranged from 48·7 for Mathematics to 36·1 for English. (The maximum possible score on AH 5 is 72.) The order was substantially the same as for AH 4, except that History and Classics went up from sixth to fourth place; whilst Medicine descended from fourth to sixth. The differences between the means of the Mathematics and English groups on both tests are highly significant.

It would be rash, however, to infer from these results that students reading Mathematics and Mechanical Sciences do in fact tend to be more intelligent than those reading English or Modern Languages. The members of each group are so few that they cannot be assumed to be representative, and there is no reason to suppose that the unrepresentativeness works in the same direction for each faculty.

Even if the data were less inadequate, and we were able to say with some certainty that the mathematicians, for instance, tended to gain higher scores on these tests than the English students—we should not know how to interpret this result. For instance:

(*a*) It might be due to the fact that the kind of intelligence required by a mathematics student is more closely allied to the kind which lends itself to mental testing, than is the kind required of a student of English.

(*b*) It may be that a *training* in mathematics facilitates manipulation of the kind of symbols, and familiarizes the subject with the conventions, used in intelligence tests, whereas a training in English is quite unrelated to these particular methods—or even produces negative transfer.

It was thought that the relative positions gained by the different faculties on the separate parts of the tests might throw some light on this question. Mathematics and Engineering were consistently near the top on both Parts of both tests. However, History and Classics, combined, came third on Part I of AH 4 and seventh on Part II. In *AH 5*, their difference was still more marked, diving from second on Part I to seventh on Part II. The only other striking changes were found for the Natural Scientists, who leapt from eighth on AH 5 I to fourth on AH 5 II; and the Miscellaneous and English groups, who changed from ninth to sixth respectively on AH 5 I and II.

Thus there is some evidence that people reading for an Arts degree tend to do better on Part I of the tests—the Part with the verbal and arithmetical bias; this is particularly marked in the case of History and Classics—these students doing really well on both Part I's but below average on both Part II's.

A comparison of men's with women's AH 4 scores showed, as has been found on other tests, that the women tend to do relatively better verbally, and the men, visually. This did not hold, however, with AH 5. It was interesting to observe that the women gained a better overall score on AH 4, but the men gained a better overall score on AH 5. The sex differences in all cases were slight, however, and little importance should be attached to them in view of the discrepancy in size between the two groups.

V. Summary and conclusions

1. The main objections to existing tests of high-grade intelligence have been outlined, and the principles of construction of Test AH 5 described.

2. An enquiry into some of the problems of high-grade intelligence has been carried out: 180 Cambridge University students of varied faculties have taken Intelligence Tests AH4 and AH 5, with and without a time limit.

3. Analysis of these test results has yielded the following results:

(*a*) AH 5 Limited correlates far more highly with AH 5 Unlimited than does AH 4 Limited with AH 4 Unlimited. This suggests that AH 5 is less of a speed test than AH 4, when applied to a highly selected, intelligent group.

(*b*) A tendency was found for the subjects who prefer to work fast to score better (on both tests) than the subjects who work more slowly for choice.

(*c*) The correlation between the two Parts of the tests was found to be highest on AH 4 Limited and lowest on AH 5 Limited. This suggests that, at the higher levels, differences in bias may play an increasingly important role.

(*d*) The data on different university faculties were inadequate for statistical purposes. A tendency was found, however, for mathematicians and engineers to score consistently highly, and modern languages and English students to score consistently poorly.

4. Future enquiries will, it is hoped, include:

(*a*) Testing a selected *low-grade* group on AH 4 Limited and Unlimited in order to see whether the correlation between the two sets of scores is higher than that found on AH 4, and as high as that found on AH 5 with a high-grade group.

(*b*) The lecturers and research workers who took AH 4 are to be retested on AH 5. If their scores are significantly higher on AH 5 than were the scores of the students, this will imply that discrimination among highly intelligent adults, by means of psychological tests, is demonstrable, even when the more intelligent subjects are appreciably older.

(*c*) Representative groups of students from different universities should be tested on AH 5, in order to obtain further evidence on the existence and extent of differences between the various university faculties.

The writer wishes to express her thanks to Miss V. Horn for her help with the testing and the analysis of results, and to Mr E. G. Chambers for his invaluable advice and assistance on the statistical side.

APPENDIX

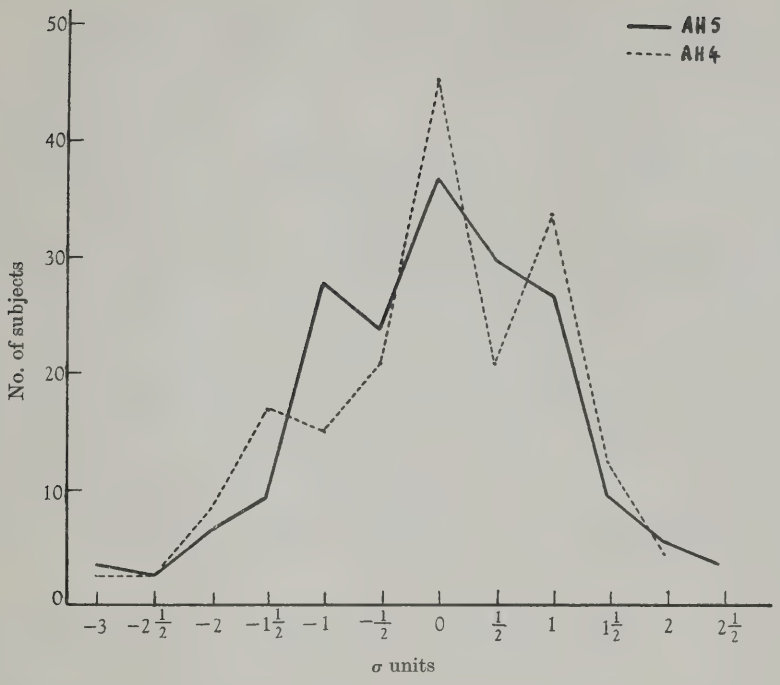


Fig. 1 shows the frequency distribution curves for Tests AH 4 and AH 5 (Limited), for the 165 students who took both tests. AH 4 yields a symmetrical, uni-modal distribution *when applied to the unselected groups* for whom it was devised. It may be seen, however, that when taken by a group of highly selected, intelligent subjects there is a tendency towards bi-modality and slight bunching at the good end. On the other hand, the same group, when given AH 5, produces a curve that is less skewed and generally smoother.

This provides some confirmation of the hypothesis that AH 5 is a more appropriate test for selected subjects of high intelligence.

ON PHYSIOLOGY AND CONSCIOUSNESS

By M. H. PIRENNE, *from the Cambridge Psychological Laboratory*

In the latter half of the last century Helmholtz (6,7,8) put forward, on apparently purely scientific grounds, the view that our representations of the world cannot be anything but symbols, natural signs of the external things, which we learn to use for regulating our actions. His argument has exerted considerable influence and is not infrequently used nowadays. Helmholtz based his thesis, which had also been advanced by others on philosophical grounds, on Müller's principle of the 'specific energies of nerves'. He apparently considered this principle to be a purely physiological one; but it may be said that, besides physiology, Müller's principle involves a definite metaphysical conception of the relation of body and consciousness, and that Helmholtz's view thus depends on this particular conception, pure physiology being insufficient to prove it. Our knowledge of the nervous system has grown since the time of Helmholtz, while certain trends have become more apparent in the methods of approach used in physiology. A brief re-examination of the problem thus seems worth attempting, and may help to clarify some questions of methodology.

The principle of the 'specific energies of nerves' was fully formulated in 1840 by Johannes Müller (9). 'Energy' means here, not physical energy, but the capacity to evoke a certain quality of sensation in consciousness. Müller said that, in whatever way a given nerve is stimulated, it will always produce the same kind of sensation, characteristic of this nerve. He seemed to incline towards the view that this specificity was a property of the central ending of the nerve in the brain.[1] The same kind of external stimulation applied to different sensory nerves produces different qualities of sensation. When the senses are functioning, it is not the qualities of the external objects which are conveyed to consciousness, but the 'specific energies' of the sensory nerves, released as the nerves are stimulated by the objects. Hence, Helmholtz argued, there is no relation between the nature of external things and the nature of the sensations and perceptions which they produce in our consciousness. Our representation of the world can only be symbolic. Piéron (12,13), in entire agreement with Helmholtz, recently declared that it must be as far removed from reality as the word representing a sensation is removed from the sensation itself.

As is well known, modern methods of electrical recording show that nervous communication of all kinds is carried out in the same way by a succession of brief impulses. All nerve fibres conduct impulses which are fundamentally alike. Variation in the impulse frequency is the only form of gradation of which the activity of a fibre is capable (1, 2). This confirms the main physiological idea contained in Müller's principle, in that it shows that the nerves behave like indifferent conductors, the diversity observed in the results

[1] Johannes Müller wrote in his *Handbuch* (9) under the headings "V. Buch, Von den Sinnen; Nothwendige Vorbegriffe" (vol. II, p. 261): "VII. Ob die Ursachen der verschiedenen Energieen der Sinnesnerven in ihnen selbst liegen, oder in Hirn und Rückenmarkstheilen, zu welchen sie hingehen, ist unbekannt, aber es ist gewiss dass die Centraltheile der Sinnesnerven im Gehirn, unabhängig von den Nervenleitern, der bestimmten Sinnesempfindungen fähig sind." There follows about one page of explanations and discussion.

of nervous stimulation depending upon the central structure to which the nerves are connected. When the sense organs are functioning, therefore, large numbers of impulses are continually reaching the central nervous system. If the external stimulus is varied, different sets of sensory fibres come into action on account of the selective sensitivity of the various peripheral receptors. The nature of the nervous excitation, however, remains always essentially the same.

In what concerns the present philosophical problem, it must be observed that, strictly speaking, this modern information does not relate to sensation and consciousness. The sense organs have been studied by purely physiological methods and it is logical to use the same methods when the central nervous system is reached. What the physiologist observes there, is a spreading of the nervous excitation pattern to other structures—which may already be spontaneously active—and eventually to motor nerves. These then put muscles into action, which, for instance, make the subject speak and say: "I have a sensation of light." The scalpel of the physiologist does not find the consciousness in the brain. As long as he adheres strictly to the discipline of physiology, it seems that he is only concerned with things such as nerves and muscles, molecules and quanta, and that he can only study response and behaviour. Thus it appears that, although it may study the conditions of the behaviour which is called 'conscious', physiology *as such* disregards consciousness. It may, therefore, be argued that to use consciousness, or the mind, as one term of a reasoning, and the outside world as the other (as was done by Müller and Helmholtz), amounts to abandoning physiology proper for a metaphysical system.

It is interesting to note that, as early as 1637, Descartes (4) had elaborated to a remarkable extent the very ideas discussed here. In his *Dioptrique* he explains how a picture of the external objects is projected on to the retina of the eye. The optic nerve then transmits to the brain a pattern of 'movements', bearing a resemblance to the retinal image, and, thus, to the external objects. It is *not*, however, by virtue of this resemblance that we have the sensation of seeing these objects, as would be the case if there were in our brain other eyes with which we could see the image. "Mais plutôt que ce sont les mouvements par lesquels elle est composée, qui agissant immédiatement contre notre âme tant qu'elle est unie à notre corps, sont institués de la nature pour lui faire avoir de tels sentiments." Descartes gives no precise description of the way the nervous excitation is thus transmitted to the soul. But he insists on the point that there need not be any resemblance between the ideas in the soul and the movements in the brain which cause these ideas. For, when someone receives a blow on the eye, it seems to him that he sees light, while the same blow on the ear would make him hear sound. And Descartes develops what amounts to the principle of the 'specific energies' of nerves.

It is necessary here to examine more closely the methods of sensory physiology. For sensations and consciousness are mentioned in physiological works. It must be seen whether physiologists *qua* physiologists have found it necessary to introduce these concepts in their reasonings, or whether they are used merely as a manner of speech.

It is generally recognized in physiology that, in the words of Adrian (2), "arguments which jump from the material plane to the mental may land us in all kinds of error". And among many zoologists it has become an accepted principle that they investigate behaviour and response only; they do not refer to the sensations of a bee or of a fish. It is understandable why zoologists have had to become cautious in this respect. First, the

confusions of anthropomorphism must be avoided. Some animals have receptors simila to ours, while their nervous system has quite a different structure (jellyfish); it cannot b assumed that the receptors of these animals mediate sensations similar to ours, if at all Secondly, many animals have receptors of a kind which does not exist in man (lateral lin organs of fish); it is even clearer here that we cannot discuss the sensations which thes animals may have through the medium of such organs. The modern approach, therefore is to study only the modalities of stimulus and response, relying on a generalized form o the principle of reflex action (10), due account being taken of the 'autonomous' activity (1 of the nervous system.

Sensations, however, are mentioned in physiological and even in some physical context in connexion with Fechner's so-called psycho-physical law (5), which purports to give measure of sensations. What Fechner and his followers do is to measure the intensity o physical stimuli which produce certain definite responses in the subject of the experiment and to introduce these values into a particular mathematical equation. The result is calle the measure of the 'sensation' given to the subject by a certain definite stimulus. Now on whatever theoretical grounds the equation may be chosen, it is evident that the resul is, in the most general terms, a mathematical function of the measurements of the stimul and, possibly, of the responses. As J. Tannery (16) pointed out, this is simply a *definitio* of 'sensation', as legitimate as it is arbitrary. Fechner's 'sensation' is defined essentiall in the same way as, say, temperature is defined in physics (from the yield of a reversibl thermodynamical cycle). It is a physical magnitude, like the height or the weight of man. It cannot be said that this 'sensation' refers to the consciousness of the subject for the same operations might be performed on an automaton instead of a human subjec and a value of the 'sensation' of the automaton would then be obtained. Fechner' method in its original form gave the logarithm of the stimulus intensity as the measure o 'sensation'. It is worth noting that functions such as logarithm of light or sound intensit are very useful and are constantly in use in sensory physiology, because the magnitud of many *responses* is proportional to the logarithm of the intensity of the stimulus, at leas within limits.

As for the investigations which search for the 'seat of consciousness', it again seems clea that, strictly speaking, they merely look for those nervous structures whose prope functioning is necessary for certain well-defined types of response and behaviour (11).

Finally, as an example showing that the introduction of consciousness and sensatio into physiological reasonings may be confusing, the problem of binocular vision may b examined. When we look at an object with both eyes, we are, in general, conscious o seeing one, not two, objects. Although the stimulation is double, the sensation is single Clearly it is this which has led to the supposition that the two nerve fibres issuing fort from corresponding points of the two retinae actually unite so as to form a single fibre a some level in the brain. This supposition is widespread, notwithstanding the objection which have been levelled against it by Helmholtz (7) and others. In that connexion, th following argument, based solely on the analysis of behaviour, may be put forward. Th behaviour of a person trying to seize an object (without having clues about its distance is very different when one eye or when two eyes are used, the subject of the experimen being much more successful in the latter than in the former case. A *reductio ad absurdu* may now be effected. Assume for a moment that the two nerve fibres originating fro

corresponding points of the retinae become united so as to form a single fibre, as stated above, and that this fibre connects in some way with the motor centres. More explicitly, this means that the impulses in the two fibres which come from the two eyes would impinge upon one single cell, which would itself discharge impulses in the usual way; these impulses then would impinge upon other cells, so that motor cells eventually would become excited. Under such conditions, the only possible difference between the results of uniocular and binocular stimulation would be one of intensity, and the behaviour could not be qualitatively different. The behaviour, however, is observed to be different. The anatomical and physiological arrangement postulated above therefore cannot exist. Prima facie evidence derived from the study of behaviour thus disagrees thoroughly with prima facie evidence derived from the consideration of the sensations in our consciousness.

Now, in recent experiments (14) on the binocular absolute threshold, it was found that there was no physiological summation of the light stimuli falling on the two eyes. The outcome of the experiments was the same as if the eyes were independent of each other, as if belonging to two different persons. The conclusion at the time was that the barrier to be overcome by the stimulation must be situated below the fusion level of the nervous paths coming from each eye: this would become obvious if there were *no* fusion level. The experiments of Sherrington (15) on flicker fusion in binocular vision, and such phenomena as stereoscopic lustre and the fact that the right and left pictures in a stereoscope cannot be interchanged without completely altering the appearance of the relief (8), are in keeping with the view that there is no binocular anatomical fusion.

Anatomical studies on the monkey (3) show that the various retinal spots are connected to the cerebral cortex by paths which remain independent of one another. The situation is more complex than might have been supposed, for each optic nerve fibre at the level of the lateral geniculate bodies connects with five or six parallel fibres which go separately to the cortex; there is, however, no evidence of anatomical connexions between the pathways corresponding to different optic nerve fibres. In the cortex, the pathways originating in the two eyes come very close to one another, but the actual connexions are not well known. So far, therefore, there is no positive evidence of anatomical fusion, of the kind postulated above, between the nervous structures belonging to the two eyes.

This rapid examination of a complex problem may suffice to show that the introduction into physiological reasoning of the sensations arising in consciousness may be misleading (even apart from the difficulties of introspection), since it has led to the acceptance by some of a mechanism the existence of which would apparently make obvious facts of behaviour impossible.

From the preceding discussion it follows that the introduction into physiology of such concepts as consciousness and sensation is unnecessary and sometimes misleading. The method of approach explicitly defined by the zoologists indicated above, and which disregards consciousness, seems more accurate and fruitful. It appears, therefore, that the concept of consciousness, wherein alone can knowledge be found, does not really belong to physiology, for which reason the purely physiological study of the nervous system never reaches the plane of 'knowing' at all, and consequently can neither support nor refute the view that our representations of the world bear no relation to the nature of the world. This view rests on supplementary assumptions regarding the relation of mind and body, such as were made by Descartes.

This is not the place for a metaphysical discussion, but it may be pointed out that i does not seem possible to consider the physiologist's approach as being anything mor than a particular way of looking at the world. For, if it were accepted as a complet account of the world, that is, if it became a metaphysic, it would follow (as has ofte been pointed out) that there existed no mind or consciousness anywhere, not even in th philosopher who developed such a thesis. Such a philosopher's speaking or writing woul merely be meaningless muscular movements.

I am indebted to Professor F. C. Bartlett for his interest in this note, but I alone ar responsible for the opinions expressed in it.

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THE RECALLING OF THOUGHTS

By H. F. FRIEDLANDER

(1) *The problem* (p. 87). (2) *Can thoughts be recalled without any form of imagery?* (pp. 87–88). (3) *Formulation and recall* (pp. 88–89). (4) *The mechanism of recall* (pp. 89–91). (5) *The function of words* (pp 91–95). (6) *Summary* (p. 95). *References* (p. 95).

(1) The problem

The problem of the present paper is: *Can we think without images?* Aristotle said that such thinking is impossible. But scholasticism and Cartesian psychology maintained the existence of pure thinking, while Locke and the empiristic school of philosophers agreed with Aristotle. Discussion entered a new stage when the problem was tackled by the modern psychology of thought-processes as inaugurated in Germany by Külpe and his followers in the first decade of this century. These investigations, based on controlled introspection, concluded that the existence of *imageless, unsensuous thought-content* cannot be doubted. This view, anticipated by Binet and Stout, was approved by many psychologists, for instance, Aveling and Spearman in this country, and Woodworth in America. But there were also strong criticism and opposition, from, for example, Wundt and G. E. Müller in Germany, and Titchener in America.

The following is an attempt to approach the problem of imageless thought by considering the process of the recall of thoughts, thought being considered, not as an act, but as mental *content*. There are two questions which have to be answered. First, can a thought be detached so completely from its verbal expression and sensuous representation that we can recall it without the support of any images ('imageless recollection of thoughts'[1])? Secondly, how is it to be explained that a thought can separate itself in our memory from the verbal expression in such a manner that we can recall it expressed by other words than the original words, which for their part can no more be remembered?

(2) Can thoughts be recalled without any form of imagery?

I have never succeeded in observing completely imageless recall of thoughts. In memory we may be *directed* towards a thought without being able to express it, but this does not mean that we can really recall a definite thought in an unsensuous way. It means that the content of the thought as such is not represented in consciousness at all, but that we can remember the thought indirectly by the help of subsidiary images which determine it by local, temporal or other relations. In this way I may remember a sentence by recalling its place on the top of the previous page, or by recalling a certain person as speaking that sentence on a certain occasion. Such images or ideas may be called *indicative* because they do not represent the object themselves (object-images) but only give an indication of them.

[1] By 'imageless' is meant 'without images' (including verbal images) and, of course, without sensations which *support or represent the thought in question*. There may be at the same time images or sensations which are in no relation to the thought, not even in a symbolic relation, just as there are always peripheral sensations that are not connected with a simultaneous thought at all. But they have nothing to do with our question.

We can then speak of images or ideas of *direction* when we are directed towards a thought without a direct representation of it. As the thought itself is not in consciousness, these cases do not justify the assumption of an imageless recall of thoughts.

An instance in point is the observation by Külpe in Bühler's investigation on thought-memory (1) (p. 37). The sentence read to the observer in the second series was "Wild in youth, wise in old age". The sentence of similar tenor in the first series which was remembered was "The worse the student the better the parson". "*I was instantly directed towards the former thought but had to search for the words*" (italics mine). "Here is a consciousness of identity; one is aware of having had that thought before; one knows it was the same, only in other words."

Another case is taken from my own records. I was given the sentence: "We may not believe in flattery, the flatterer wins us nevertheless." The comment was: "It reminds me of a thought expressed once by a friend of mine, *although none of his words are remembered by me.* I know, however, that he has expressed such a thought. On further considering it I remember now it was about students praising him."

Thus, whenever observers say that the thought of a sentence that they have heard before is clearly recalled by them and that they lack only the words for which they have to search ((1), p. 59), this would not appear to mean that the thought is present to them separated from its words, as an *independent and imageless experience.* It is merely a turning of our minds to something past, which remains undeveloped as to its content. Sometimes there may arise an additional feeling of certainty of being able to recall its content.

This internal look, which turns to the past in a definite direction but without any direct representation of the content to which it is directed, only *touches* the traces of memory, and this is a psychological fact of great importance for the mechanism of remembering. If I want to recall something, such an inward direction is often sufficient to put in motion the mechanism of recall, and we do not need the assumption of imageless thoughts to understand this mechanism.

In order to observe the process of the recall of thoughts I read short tales (2), each of them three times through, and tried to reproduce the story after a pause of five minutes. One of my records was as follows:

While I am reproducing the tale I am aware of *being directed* to it in my mind. This kind of attitude is analogous to that of turning my eyes in a certain direction. The recall of thoughts proceeds under the influence of this direction. The tale that I wish to recall is in my possession, and I feel sure I can recall it as soon as I please. Before I reproduce single sentences, there emerge sometimes images of what I have read before but by no means with every detail. I cannot discover any imageless thought. Within the general direction towards the tale the internal glance proceeds from point to point, as if we should look in a certain direction of space and focus our eyes step by step on successive points as in pursuant movement. A look directed in this way is often sufficient to make the right sentence arise.

In none of these instances, then, is there any direct recall of a thought separated from its verbal expression. There is only a direction of the mind towards a thought that is individualized with reference to its time, place or in some other way, but for the rest absolutely void of content.

(3) Formulation and recall

How does this direction of the mind towards former thoughts enable us to recall them, though not, perhaps, in their original form? I set myself the task of reproducing a passage just read (without looking again into the book) unchanged as to its content but worded differently. I found that I could achieve this, partly by the help of phrases which I did

directly remember, partly through a feeling that I should certainly be able to fulfil the task. In everyday life the same thing happens. When I deliver a lecture the sentences employed are not usually those which I have written but they convey the sense of the prepared text. I am accustomed to give my lectures in this way, and a very short 'glance' of my mind at the sentences elaborated before is sufficient to produce an entirely different way of conveying the same ideas.

Many years ago von Kries wrote: "We read, e.g. that the Russian ambassador has been withdrawn from London. How is it to be explained that, in the process of remembering this fact, we are just as likely to recall it in its original form or in the quite different form that Russia has broken off diplomatic relations with England?" (translated from (3)). Those who believe in imageless thought find the explanation easy. They say that thoughts, as unsensuous contents, are independent of utterance and, therefore, can be revived in a different form as easily as with no words at all. We cannot accept this, since we have denied that there is sufficient direct evidence for imageless thoughts. But we must ask whether, even if they cannot be directly observed, imageless thoughts are, nevertheless, an indispensable hypothesis for explaining the recall of thoughts with a change of words. If they are, we have a right to expect that the hypothesis will some time be confirmed by direct observation.

We call a sentence which was originally perceived S', its thought T', the memory traces of S' and T', S'_t and T'_t, their recall S'_r and T'_r. We know that T'_r is separable from its original S'_r, but from this it by no means follows that T'_r is separable and independent from *any* S_r. It could perhaps be assumed that the trace T'_t might be disconnected from any S_t, at least for a short time, till it became attached to a new S''_t. But as long as scrutiny does not reveal a corresponding independent T'_r and since in any case this particular assumption is not the only one that can explain the facts, we have no reason to use such a hypothesis.

The recall of thoughts with their wording changed can be explained much better by assuming that S'_t and T'_t form a unity from which T'_t cannot be detached. This unit $S'_tT'_t$ would then have to be considered as variable with regard to S'_t or to T'_t or to both of these components, and this would explain the recall of T' with a new wording S''_t.

This hypothesis is in closer harmony with the modern view of memory than any other assumption. A long series of experiments led F. C. Bartlett to the opinion that memory does not consist of fragmentary and lifeless fixed traces but of 'organized settings' of living and changing traces (4). Thus, in accordance with this view, it is much more likely that the traces of a sentence and its thought form an organized setting rather than an association from which the trace of the thought (T'_t) can be split off at any time; and consequently it is more likely that S'_t and T'_t change as parts of a whole than that separated T'_t takes hold of a new verbal expression (S''_t).

(4) The mechanism of recall

The facts are altogether in favour of the view that S_t and T_t form an inseparable unity. Let us recall single sentences or whole stories, and compare the phrasing of memory with that of the original text. For evidence concerning single sentences we can refer to K. Bühler and on both sentences and narrations to F. C. Bartlett (4) and H. G. Hartgenbusch (5), who are concerned with the fidelity with which thoughts can be reproduced.

In the recall of stories numerous sentences are found in which neither the original thoughts nor their verbal expressions are retained. Changes may be of omission, addition transposition and transformation of phrasing. Sometimes essential thoughts are altered but in most cases the changes are in more or less insignificant details. Bartlett and Hartgenbusch show that the changes occur while the original unity between the two series ΣS_t and ΣT_t concerning the context of the whole story is preserved. But still this leaves the question open why some expressions of single sentences get lost, while the corresponding thoughts seem to be retained and recalled with a change of phrase.

First, there are cases of metaphorical expressions, the thoughts of which are properly recalled, but without reproduction of the metaphors. Bühler gives us some notable examples

(*a*) Sentence read in a first series of proverbial expressions to the observer, who has the instruction to listen and understand: "Sharpening the scythe does not delay the reaper." Sentence read in a second series to the observer with the instruction that he is to recall a sentence of similar tenor from the first series: "The farmer loses no time when feeding his plough-horses."

Record of the observer: "...then I noticed the general idea implied by the sentence, somewhat like this: certain actions, although taking time, do not in the long run waste it because they are essential. Then I recalled that a similar thought occurred in the former series. I do not know how the sentence ran, it must have been something with reaper and scythe" ((1), p. 50).

Here the metaphor has been forgotten to such an extent in favour of the general abstract sense that even the recalling of reaper and scythe cannot call forth the image of sharpening

(*b*) Original sentence: "German writers, we easily let you starve, but cheer up, we bury you in clover. [This means: "Little bread during your life, but many flowers when you die."]

Later the catch-word 'in clover' was given, and the observer was to try to recall the complete thought.

Record: "The sense I know perfectly well, it is that they leave geniuses to starve, but, after their death, they erect monuments to them. The words I do not know" ((1), p. 43).

Here the metaphor has been entirely forgotten, for the observer, asked by the experimenter how the word 'clover' was placed in the sentence, answered: "I do not know, perhaps it had something to do with heirs who profit by it."

The explanation of such cases seems to me this. The original sentence is apprehended not only as a metaphor but essentially as a general abstract proposition which occupies the whole interest of the observer; therefore, later he can only recall this proposition. This proposition may in fact have already found its own verbal expression in the observer's mind while he was comprehending the metaphorical sentence, or it may have appeared only in indistinct ideas of direction. In the latter case the new wording would not appear before the thought is recalled.

Secondly, there are the uses in recall of synonyms and paraphrases. For example, the sentences: "One of the young men said: ...I will not go along....My relatives do not know where I have gone" were recalled as follows: "One of the young men refused to go on the ground of family ties..." ((4), pp. 65–6).

Why do we, in remembering, often change constructions, e.g. transform direct speech into indirect speech, passive voice into active voice, alter phrasing and style? The reason often is that it is more difficult to impress on our minds an *unusual* form of expression than a usual one; the unusual will be easily replaced by the usual, which, before its reproduction, must have been in a state of subconscious preparedness or subexcitation. Of course, this does not prevent the unusual manner of expression from being occasionally preserved, for it may have attracted particular attention. Another reason may be that a *complicated* form of expression cannot easily be retained and will be displaced by a *simple*

form.[1] We know that analogous facts of simplification have been observed in the process of remembering optical figures and melodies (see (6)).

Whatever may be the causes of these transformations in memory, our hypothesis of a unity of S_t and T_t will always prove sound. This unity can be assured though S_t and T_t are both changed. Strictly speaking, when a sentence or a group of sentences is changed, the thoughts expressed by them will generally change correspondingly, and vice versa. Why not admit the same unity, if, in a less strict sense, we say that only the verbal component S'_t has changed into S''_t? In fact, there can be no objection against such a partial change of a complex. If a simple content such as, for example, a tone can change in one dimension (e.g. its intensity), the others (pitch and timbre) remaining constant, why should not a complex whole change in one of its components only without its unity being destroyed?

So far we have been concerned only with a change of S_t while T_t persists. But it is also possible that S_t persists, at least to some extent, and T_t is altered. We can, indeed, observe this, although not so frequently. Take as an example the following observation from Bühler's work:

> The original sentence was: "One saves at the tap but leaves the wine running through the bung hole." The observer reproduced: "There is no tap in the bung hole", and remarked: "It is like a recollection of words without sense. I do not remember what meaning they had" ((1), p. 72).

What happens here and in similar cases is this: T is forgotten—its traces (T_t) may have disappeared, or not be re-excitable—and with it S, but only as a whole that corresponds to T. Single words of S_t, however, remain and can be reproduced. These words are, of course, not meaningless, and, somehow put together, convey a different thought from the original T.

It is interesting to examine whether from certain pathological facts we can derive arguments in favour of our view. I am thinking of observations made by several psychiatrists in cases of speech disorder: the patient substitutes for words he wishes to remember others of similar meaning, and cannot recall the words which had been actually given before. Sometimes he cannot even repeat a word immediately after he heard it, but replaces it by another word of similar meaning (7,8). K. Goldstein makes the following remark about these facts: "Hence I concluded that experiences of speech are very closely related to experiences of things, and therefore either of these groups of experiences can hardly emerge as isolated and independent phenomena" (9).

(5) The function of words

The problem concerning the dependence of thought upon its verbal expression raises the question: How far do words help in impressing thoughts on memory? Is the retention of thoughts influenced by the way in which they are expressed; or are thoughts so independent that they can be equally well retained whatever their wording may be? When, in a series of observations, I was comparing the meaning of two passages in different languages, it struck me that I achieved the results with greater certainty when the passage given in my mother tongue (German) came first and less well when it came second. The chief reason for this seemed to be that the foreign passage was not so well remembered as the German passage.

[1] Hartgenbusch ((5), 278) states that one of the factors that transform memory is simplification, which is defined by him as an 'operation of a purely formal tendency', 'which aims at rendering the structure as simple as possible'.

This result may be thought to be a matter of course, the greater familiarity of th mother tongue being favourable to its better retention. But this can be so only if a stron dependence of thought on verbal expression is admitted; and it is obvious that suc dependence is best secured if one adopts the view that a thought is represented by th meaningful functions of the words and so closely tied to verbal expression.

If, however, we assume that the meaning of a sentence is an independent imageles thought, we cannot understand without further assumptions why thoughts once appre hended should not be retained as easily when expressed in a foreign language as in th mother tongue. It follows that imageless thought can be experimentally investigated i we compare the recall of thoughts which are expressed in the mother tongue with that o those expressed in a foreign language.

The conditions for such an investigation are not simple. The foreign language, bein less familiar, demands more effort of understanding. This additional energy, it may b argued, is not available for impressing the thoughts of the foreign sentences on our minds Therefore the defender of imageless thought could not expect, either, that the recall o thoughts expressed in a foreign language would be the same as that of thoughts expresse in the mother tongue. On the other hand, the greater difficulty in apprehending sentence in a foreign language may intensify the attention paid to them, and this is perhap favourable to their recall. Thus we may expect that a foreign language has a favourabl or an unfavourable influence on the recall of thoughts, or the influence may even work i both directions at the same time. If, therefore, the investigation indicates that the reca of thoughts in a foreign language is as good as that of thoughts in the mother tongue, w cannot conclude, without further research, that imageless thought exists, the reason bein that the opposite influences mentioned before might counteract and compensate eac other. Nor can we, for the same reason, decisively confirm our own view if the result show better recall when the original expressions are in the mother tongue. Nevertheles the experiment seemed to be worth trying, on the chance that, although its results migh not be conclusive, they might be suggestive. I had three subjects only. All were ver familiar with the languages, but only one was truly bilingual.

I read aloud an English and a German passage to my observers, and asked them t compare the two. In one case the English came first, in the other the German. The passage were taken from G. T. Renier, *The English: Are they Human?* and K. Silex, *John Bull a Home* (both Tauchnitz editions, Leipzig). In each passage for comparison were severa sentences covering about fifteen printed English lines. For observer III, who had a ba memory, the length was reduced to half. The German text was the translation of th English except for certain alterations. These alterations were of different kinds, in mos cases omissions, additions and insignificant changes. The sentences, clauses or phrase changed in this way were almost equally long: they contained three or four main point Sometimes I made significant alterations by omitting or adding a negation and so changin the meaning of the sentence to its contrary. Occasionally I used passages without an change of meaning.

Each series consisted of six to ten comparisons which took place at one sitting. Eac series with observer III contained sixteen comparisons because, the texts were half a long as those used for observers I and II, for the reason given above. The series contained as a rule, six omissions, four additions, three insignificant and three significant changes they followed one another in an irregular order. On proceeding from one series to th

next the order of the passages to be compared was changed: namely, the order English-German (group A) was followed by the order German-English (group B). In order to make the groups A and B comparable I made the conditions in both groups as similar as possible, arranging the alterations in both groups in the same way. Whenever differences existed they were removed by giving the same passages which had been used before in group A in group B once more, and inversely, after a long interval had elapsed. The interval was long enough to eliminate any noticeable influence of the observer's memory on the present results.

The interval that separated the presentation of the two passages to be compared was 15 sec. The observer had to indicate instantly any alteration which he noticed in the second passage.

I carried out 8 series comprising 68 comparisons with observer I. The 4 series of each group (A and B) contained 34 comparisons with 18 omissions, 14 additions, 10 insignificant and 8 significant alterations. I carried out 12 series comprising 112 comparisons with observer II. The 6 series of each group (A and B) contained 56 comparisons with 56 omissions, 16 additions, 12 insignificant and 12 significant alterations. I carried out 12 series comprising 192 comparisons with observer III. The six series of each group (A and B) contained 96 comparisons with 56 omissions, 16 additions, 12 unimportant and 12 important alterations.

The results are shown in Table 1.

Table 1

Observer	Group	No. of series	No. of comparisons (total)	No. of judgements	
				Right	Wrong
I (German)	A (English-German)	4	34	27	40·5
	B (German-English)	4	34	31·5	26·5
II (German)	A	6	56	69·5	35
	B	6	56	67·5	36·5
III (Bilingual)	A	6	96	54·5	51·5
	B	6	96	49·5	53

Observer I. This observer, a German lady, 33 years old, had learned English at school from the age of 13–17, and since that time had practised it only a little. When asked about her understanding of English, she said that she translates only difficult passages in her mind. As a rule she comprehends English almost immediately, although at the same time she has faint images of corresponding German words.

In this case *group A showed worse results than group B.* The principal errors were failure to notice omissions and especially attributing additions when there were none. This perhaps indicates that the observer did not recognize phrases or single words as they had been read out to her in the first passage. The observer's impressions of not having heard certain words and phrases before were erroneous 16·5 times in group A and only 8 times in group B.

Regarding omissions, the results were as follows:

Group A: 4·5 (2) right, 13·5 wrong.
Group B: 9 (5) right, 9 wrong.

The figures in parentheses indicate the numbers of right judgements in which the observer not only remembered the sense but also the wording of the omitted part of the sentence.

I asked the observer if she was aware of any difference between the two groups as regard difficulty of comparison. At the third sitting she said there was no difference. At the sixth sitting, however, she remarked that, in general, the comparisons in group A (English German) seemed to be much more difficult than those in group B (German-English). A the eighth sitting she tried to explain this difference, saying: "When I am concerned with English passages that appear to be more or less difficult, I lack command over the language Therefore it may happen that I cannot remember the English precisely and the comparison is rendered more difficult."

Observer II. This observer had had almost the same training in English as observer I Nevertheless, the results were different. She also stated that she understood the English sentences read to her either immediately or by translating them in her mind; it depended on the familiarity of the passage.

It will be seen that in this case there is no difference between the numbers of right and wrong judgements in both groups. And the same is true when we consider omissions additions, etc., in both groups.

At the fourth sitting the observer said spontaneously that the comparisons of group B (German-English) demanded less effort than those of group A (English-German), as the English passages of group B were easier to understand than those of group A. Of course the understanding of the English passages when heard after the German passages is facilitated because they convey the same sense as the German passages except for the alterations interspersed amongst them. At the seventh sitting she repeated the same remark, again spontaneously, namely, that group B was easier for her because the English did not require the same effort.

Observer II was especially asked, as also observer I had been, to follow the English and German passages as far as possible with equal attention. Nevertheless, we can assume that because of the greater difficulty of group A, observer II involuntarily paid more attention to the English passages in this group than to the German passages in group B According to our assumption mentioned above, it is probable that this helped to impress the English passages on her memory and compensated for their greater difficulty, and this would furnish an explanation for this observer's equal achievements in both groups.

Observer III. In order to eliminate the differences in apprehending the passages, I tried a third observer who was *bilingual.* "As far as my memory goes back, I spoke both languages (English and German)", she remarked. At the time of this investigation she lived in Germany and had been living there for many years. In her own opinion English and German were at that time equally familiar to her, although in more difficult discussions she had somewhat more command of German.

As will be seen the results showed *no difference between the two groups.*

So far as omissions, additions, etc. go, the two groups compare well except for judgements based on erroneous impressions of entire unfamiliarity. In group A this observer made 5·5 mistakes in not recognizing a sentence which actually had been given her in the first passage, and only one such mistake in group B. This shows a slight tendency in favour of group B (German-English).

This observer remarked at the second sitting that she found group B (German-English) harder than group A, and that the German made less impression, but she could not explain why. As she did not, however, mention this again on the following days, it is doubtful whether this feeling was preserved during these sittings.

As I said before, we cannot definitely conclude for or against imageless thought contents from these few results. But they may be suggestive for further research with bilingual persons, and also with those who speak two languages which are not so closely related as English and German.

(6) Summary

The foregoing study is a contribution to the discussion of the old problem: Can we think without images? The problem is approached by considering the process of recalling thoughts. The word 'thought' means here a mental content.

I. Introspection shows that there is no direct recall of a thought separated from its verbal expression or any other sensuous support. Contradictory observations seem to confuse the recalling of the thought itself with the direction of the mind towards a previous thought with the help of indicative images.

II. We often forget the original wording when we recall thoughts and replace it by new expressions of similar meaning. In order to explain this fact it is by no means necessary to assume that memory-traces of thought contents can be separated from any traces of words. A much better explanation is provided by the assumption that the traces of thoughts remain in close connexion with those of words, and form a unity with them which may be changed with regard to the words or to the thoughts or to both.

III. An interesting problem concerning the dependence of thought upon its verbal expression arises when we ask whether the retention of thoughts is influenced by the way in which they are expressed. A method is indicated which enables us to compare the retention of thoughts which are expressed in the mother tongue of the observer with the retention of those which are expressed in a foreign language. This method was applied to three observers (one of them bilingual) and the results were analysed as a first approach to this problem. Further investigations with bilingual individuals might give us an answer to that problem and provide a new argument for or against the existence of imageless thoughts.

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THE PRESENT HOUSE OF COMMONS: ITS EDUCATIONAL AND SOCIAL BACKGROUND[1]

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I. *Introduction* (pp. 96–97). II. *Method* (pp. 97–99). III. *Results* (pp. 99–105): (1) *Education*: (*a*) *Schools*, (*b*) *Universities*, (*c*) *Degrees*, (*d*) *Evening Classes*, (*e*) *Scholarships* (pp. 99–102); (2) *Social background*: (*a*) *Father's occupation*, (*b*) *First employment*, (*c*) *Present occupation*, (*d*) *Business interests*, (*e*) *Local Government* (pp. 102–105). IV. *Summary and conclusions* (pp. 105–106). *References* (p. 106). *Appendix* (pp. 107–108).

I. Introduction

In 1940 Barbara Wootton published an article (7), "Some aspects of the social structure of England and Wales". This attempted "to form as realistic a picture as possible of the class structure of our society, and to test, against this, the validity of opinions commonly held about the meaning and relationship of classes". When the present investigation was planned (1945), we were particularly interested in the so-called ruling class, since we were observing the social effects of the propaganda preceding the General Election. It therefore seemed profitable to repeat and extend Mrs Wootton's investigation in order to obtain comparable facts for the post-war Britain. After some consideration and seeking of expert advice, we decided to use the questionnaire method, which, we thought, might obtain material not available to Mrs Wootton or to Dr J. F. S. Ross (5) in his important survey of the House of Commons during the inter-war period.

The present aim is not to discover facts which might interest the party politician, but social facts from which psychological data can be obtained. In a democratic society such as ours, an understanding of its rulers and of the social background from which they come is of immense importance to the social psychologist. The ruling class is largely responsible for the making of laws and policies for, and on behalf of, the total community. Factors which cause such laws to be made and accepted or rejected by the community can frequently be discovered in the nation's past and present culture-pattern. "The laws of any country...important to persons living both inside and outside its boundaries, geographical and national...often reflect the creators of such laws. Law-makers often reflect the particular culture-pattern to which they have been subjected." (6)

It may be suggested that, before undertaking this investigation, a definition of our rulers might have been sought and that the persons to whom our questionnaire was sent are not the whole of the ruling class, if there is one. In reply, the vagueness of the present definitions of 'ruling class' must be emphasized. The present questionnaire was sent to members of the Government and House of Commons, Officials of the House of Commons, Judges, Heads of Civil Service Departments and one or two under them, Stipendiary and Metropolitan Magistrates and Recorders. These persons do not represent completely the British ruling class. No one can read, for example, Abshagen's book (1) without realizing that, in our country, the most influential rulers may not be those on our mailing list.

[1] Based on papers read at the British Psychological Society's meetings at Durham, April 1946, and Manchester, June 1946.

The group we chose to study is, in fact, a subsection of the rulers of our country. It is a 'discernible' section of the total group of rulers; those obviously involved in the formation and administration of our laws and policies. Behind this group may be further subgroups, the names of whose members may be unknown to the general public. These groups would possibly regard an overt inquiry as unnatural, even impertinent. Different attitudes of the recipients towards the questionnaire perhaps merit a special study.

The ineffectiveness of many definitions of 'ruling class' in this country may be due to the framer's lack of recognition that our rulers may fall into one of three major subgroups:

(1) Those who appear to derive power from the following factors: birth, wealth, and professional ability. They are seldom well known to the public and their influence is difficult to assess.

(2) The discernible administrators of our laws and policies. These, with a few exceptions, are covered by the present research.

(3) A ruling class, which helps to create public opinion. It is made up of persons privileged by reason of birth, wealth or education. They are assumed to be our rulers either because of a traditional culture-lag or because modern propaganda keeps their names before the public.

The inability of many persons to distinguish between these three categories has resulted in vague definitions of the ruling class and correspondingly vague valuations of it in public opinion.

At this stage our inquiry is not concerned with how our rulers came to the House or why they came, but whence they came. When our returns are examined in relation to the census-occupational and census-educational groups of the country, it is found that the present House is drawn from a larger number of the different educational and social levels of the country than recent Houses. In the past most investigators have tried to find the extent to which the House was *politically* representative of the country. That is not our present task. Though it is unjustifiable to state that in all cases similarity of educational and social background implies similarity of political allegiance, there appears from our returns to be a tendency towards this. There is a distinct educational and social path for each party: yet to state that, because the paths of the present Officials and of the Conservatives agree consistently, the Officials' political affiliations can be inferred would go beyond the facts.

II. METHOD

In November 1945 the present inquiry into the educational and social background of Officials[1] and Members of the House of Commons was initiated by Prof. T. H. Pear. After considering previous research upon this subject it was decided that as far as possible the necessary basic information should be obtained direct from the individuals concerned through a questionnaire distributed by post.

Mainly owing to difficulties of time a pilot survey was impracticable and enumerators could not be employed. It was realized that the schedule would have to be simple and reasonably brief, the questions concise, and the answers to them easy to give. By went into a first job not necessarily suited to their talents or interests, and consequently

[1] These are included here to represent a small section of the Civil Service, and for purpose of comparison with elected representatives.

December 1945, after extensive inquiries and extremely careful selection of the questions, the final form of the questionnaire was decided upon.[1] Suggestions for the main categories were obtained from previous works, but, in addition, the questions (see questionnaire on p. 108) were selected with a view to obtaining more detailed information within those categories than had been available previously.

In February 1946 the questionnaires, with an explanatory letter signed personally by the Director of the Department of Psychology and a stamped addressed envelope for its return, were distributed by post. These were sent to all Officials and Members of the House of Commons, a total of 706, and it was indicated that the return of the completed schedules by the end of February would be appreciated. At the end of the month 280 reminders were issued. By June 1946 the total return of completed forms was 456, 64·7% of the total number distributed. These were then filed: the Officials of the House of Commons in a separate category, the Members of the House of Commons according to their parties in the case of Labour and Conservatives, and the Members of the remaining smaller parties (Liberals, Independents, Communists, etc.) grouped together as 'Other Parties'. Analysis of the returns according to the above categories gives the results shown in Table 1. (The results for the *officials* were as follows: Total no., 66; no. of returns, 34; percentage return, 51·5%.)

Table 1. *Members of the House of Commons*

	Labour	Conservative	'Other Parties'	
Nos. in the House of Commons	394	197	49	640
Percentage of total in the House of Commons	61·6	30·8	7·6	100
No. of returns	305	86	32	423
No. of returns as a percentage of total no. in the House of Commons (640)	47·7	13·4	5·0	66·1
Returns as a percentage of party strength	77·4	43·6	65·3	—

All further analysis of the information so far has been based on these samples. It is, however, hoped that for future research the sample percentage may be increased by use of information from alternative sources.

Even without this further information, however, it should be noted that the response on the whole has been good and that the answers were in the vast majority of cases frank and concise. It is also of importance to note that the questionnaire covered all the Officials and Members of the House. Previous inquiries either sought information from reference books or took only random samples of the population. The direct contact with all the individuals under examination, and the more detailed information obtained, suggest that the basic data at our disposal are fuller than any assembled before on this subject. Comparative studies might profitably take this into account.

Though extreme care had been taken in framing the questions during the checking of the answers difficulties presented themselves. In the 'education' category it was discovered that our definition of the types of school, grammar or public, in many cases did not coincide with that used by those who answered. It was decided to recognize as public schools those enumerated by the Headmasters' Conference of 1945, and to use this as a basis for tabulation. (This was also done by J. F. S. Ross(5), p. 42.) The answers

[1] For valuable advice we wish to thank heartily Dr Henry Durant and Mr John Gilbert.

concerning the type of degree were, in some instances, given as 'M.A.' or 'B.A.', not stating whether science or arts subjects had been studied. To clarify this, check question-forms were sent out to sixty-one persons.

In the 'occupation' category, the answers to the first question (age of first paid employment) included some which indicated only a part-time paid employment (e.g. newspaper-boy whilst still attending school). In these cases the beginning of full-time paid employment has been deduced from the information concerning education.

Another difficulty was encountered when some Members indicated their present occupation as 'M.P.' without stating whether this was their sole employment at the present time.

Because of the different sizes of the samples obtained from each party, comparison of the various parties and their attributes on the basis of the varying samples is difficult. This should be borne in mind whenever figures from the returns are quoted.

III. RESULTS

(1) *Education*

Ross ((5), p. 50) writes: "In investigating the school education of M.P.'s, one is struck not only with the big proportion of public school men in the House of Commons, but also by the marked preponderance of Eton, and to a less extent, Harrow. It will probably astonish most people to learn that roughly one-fourth of all the Conservatives who sat in the House at any time during the inter-war period were old Etonians, while one-third of the whole number were from either Eton or Harrow. The proportions of the other parties were, as one would expect, much smaller, but even so we find that one in every seven of all the members of all the parties together, including Labour, was an old Etonian or an old Harrovian. The actual number of old Etonians in the House naturally varies from time to time, but during the inter-war period the average was 105. The Harrow average was 43."

And (p. 52) "We may say that whereas the secondary school boy has, *prima facie*, about eighteen times as good a chance of entering Parliament as the elementary school boy, and the public school boy has about 115 times as good a chance, the Harrovian has over 1800 times and the Etonian well over 2000 times as good a chance, and this in spite of the rise of the Labour Party."

This forms an interesting perspective in which to view the educational background of the 1945 House of Commons.[1] To translate Ross's figures into percentages would mean that in the inter-war period the average percentage of the House of Commons who went to Eton would be 17, to Harrow 7. The percentage so far of the 1945 sample is Eton 9, Harrow 1. Many who have not replied to our questionnaire may have the old educational background, so that the change may not be as great as the above figures suggest. It is interesting, however, to note the percentage distribution in the 1945 sample of M.P.'s who attended Eton, Harrow and Winchester (see Table 2).

Table 2

	Eton %	Harrow %	Winchester %
Labour	2	0·7	1·3
Conservative	37	5	1·2
M.P.'s of all other parties	6	Nil	10·0
Officials of the House of Commons	26	3	3·1

Actually, in our sample, Winchester occupies second place. One other school comes into the picture—Wellington ties with Harrow for third place.

[1] The full tables of these results, which are in substance quoted in the text, may be obtained on application to the authors.

The education of many Members of the present House of Commons was affected by the State education available during their youth. It is therefore important to bear in mind, when considering details of the present returns, changes in the educational system which have occurred during the lives of our present Members of Parliament. The average age of our total sample is 54·2 years and that of Labour 53, Conservative 43·3, 'Other Parties' 60·5, Officials 61. The Education Acts passed between 1890 and 1921, therefore, affect the educational background to be considered, and a brief summary of the important points is given below.

The Education Act of 1880 had legislated for direct compulsion to parents to enforce their children's attendance at school. In 1891 a new Education Act laid it down that every school district must provide sufficient public school accommodation for children over 3 and under 15 without payment of fees. However, what was meant by "sufficient accommodation" was not specified and there was still no other form of controlled education. 1899 saw the Endowed Schools Act and the creation of the Board of Education. There was then a determined endeavour to promote secondary education by giving drastic powers to a Commission to make schemes for the better administration of existing endowments devoted to this purpose. Provision was also made for supplying or aiding the supply of technical and manual instruction. In 1902 School Boards were abolished and their functions transferred to Local Education Authorities. This Act dealt drastically with Higher as well as with Elementary education. The local authorities, i.e. County Councils and County Borough Councils, were required to "consider the educational needs of their areas and take such steps as seemed to them desirable, after consultation with the Board of Education, to supply or aid the supply of education other than elementary and to provide for the general co-ordination of all forms of education...." The ultimate results, to which subsequent acts and various Statutory Regulations contributed, were:

(*a*) The establishment and rapid development of secondary (grammar) schools wholly provided and maintained by Local Education Authorities.

(*b*) The granting by Local Education Authorities of financial assistance to a number of existing secondary (grammar) schools, some being taken over completely by Local Education Authorities.

(*c*) The co-ordination and further development of technical instruction, for which, under the Technical Instruction Act of 1889, County Councils and County Borough Councils had already been given some responsibility.

(*d*) The establishment of a number of training colleges managed and maintained by the Local Education Authorities.

The Education Act of 1918 did not substantially add to existing types of educational provision, though intended to do so. It finally abolished fees in elementary schools and raised the upper age of attendance to the end of the term in which the pupil reached his fourteenth birthday; and charged Local Education Authorities with the duty of providing instruction for the older children.

(*a*) *Schools.* Perhaps the most dramatic contrast in the educational background of our M.P.'s is shown in the percentage who had no school education above the elementary level. With Labour it is 43·2%, Conservative 1·3%, 'Other Parties' 6·2%, and Officials 8·8%.[1] 39·8% of Labour members proceeded to secondary schools and 17% to public schools (this is comparable with the 20·7% of Conservatives who went to secondary schools). Labour is also distinguished from the other parties by the fact that a higher proportion of its members attended central and technical schools. The normal path of the Conservative members has been from private or preparatory school to public school; 78% attended both these types of school. The results from members of all other parties show a more even distribution between the secondary and public schools; 50% went to secondary and 43·8% to public schools. The path of the Officials is almost identical with

[1] For details of figures given below, see statistical tables at end of paper. The figures refer to results from sample, not to the total number of M.P.'s.

that of the Conservatives in that they usually proceeded from preparatory to public schools; however, the percentage at public schools was even higher, 82·4, and that at secondary schools much lower, 8·8.

(*b*) *Universities.* When writing of universities Ross correlates university with school education and says:

> The older, the more expensive and the more socially exclusive the university or school, the more certainly will its product be found in the right wing of politics, and vice-versa....

Oxford is the university most favoured by the Conservative, with Cambridge next and other universities a long way behind. The Labour Party reverses the picture ((5), p. 57).

The 1945 material shows a changed situation. The influence of Oxford as a single university now predominates in all parties and is nearly equal to that of all the provincial universities put together. For the number of M.P.'s, etc., from Oxford was 29·7%; and from the provincial universities 30·6%. Examination of the percentage distribution of all those from all categories who have attended universities shows the predominating part played by our older universities in educating our administrators: Oxford 40·9%, Cambridge 20·2%, London 10·8%, provincial 23·2% and foreign 4·4%.

Although the educational experience of Labour M.P.'s shows a very wide scatter, only a comparatively small number of them attended university degree courses, 36·4%. Of those who did, 29·7% went to Oxford, 15·3% to Cambridge, 17·1% to London, 30·6% to provincial and 7·2% to foreign universities. Of the Conservative sample, 63% attended universities. Nearly half went to Oxford, 49·1%; Cambridge comes next with 29·1%, provincial next with 16·3%, and London was very low with only 3·7%. Of the 'Other Parties', 53·1% attended universities. The great majority (53%) went to Oxford, with provincial universities next in the list (29·4%) and Cambridge last with 17·6%. Some went to London or to foreign universities. With the Officials, the predominance of Oxford was very great; it attracted 70% of those attending universities. Cambridge comes next with 25%. London is a very poor third with 5%. None of the Officials who replied to the questionnaire had attended a provincial or foreign university.

(*c*) *Degrees.* Analysis of the subjects in which degrees were taken suggests that in the present House the Labour Party—largely because of its numerical strength and its recruitment from a wider educational field—has the widest distribution of educational experience. In all parties, degrees in history were numerous while science degrees were scarce.

(*d*) *Evening classes.* A comparatively small number of the Labour members attended universities, but it should be noted that they achieved their further education through attendance at evening classes, which play a most important part in the educational background of the present Labour Party.

Our returns show that:

> 49% of the Labour Party attended evening classes.
> 6% of the Conservative Party attended evening classes.
> 10% of the 'Other Parties' attended evening classes.
> 14% of the Officials attended evening classes.

The subjects studied can be divided into social science, languages, commercial, cultural, occupational and pure science. Social science was the most popular and pure science the least popular.

(*e*) *Scholarships.* It is most interesting to note the distribution of scholarship-aided education in the categories we are at present studying. Our figures suggest that uncommon views in politics seem to correlate with unusual scholastic achievement. So far as our returns show, 'Other Parties' gained a 100% success in their university education and topped the list in the attainment of scholarships; 50% of this category won school scholarships, 31·3% university scholarships, 63% other scholarships. In this section, for the first time, the educational paths of the Labour Members and Officials seem to touch, for 29·2% Labour Members and 29·2% Officials won school scholarships. For university scholarships, the numbers were Labour 18%, Officials 23·6%. The different types of school and university attended by these categories have already been shown, and the difference in educational paths is accentuated here by the difference shown in numbers winning other types of scholarships. The Officials won scholarships to public schools and the older universities only. Labour won scholarships at secondary schools, all types of university, and 14·1% attained scholarships which took them to colleges of non-university status.

(2) *Social background*

The Members of the House of Commons, whose ages range from 25 to 78, are drawn from over twenty occupational groups, most counties of England and some foreign countries, a large range of social strata, most religious denominations, a variety of schools and universities in this and other countries, and a wide range of income groups. It would be interesting to the social psychologist to see how far these factors, separately and jointly, affect the policy of the political administrator. Consequently, in framing the questionnaire, we were careful to insert as many of these categories as busy politicians could reasonably be expected to answer. It is not claimed that an analysis of these returns would provide a complete account of the effect of cultural influences upon political allegiance, for at present social scientists have no means of assessing the extent to which the whole or any part of the culture-pattern affects human personality.

The previous section demonstrated the differences of the educational background among Members of Parliament. But other factors are as important. Many of the present M.P.'s finished their school education before they were 15; some were in full-time employment at 12; some in part-time employment at 10. It may be that experience gained in their first and successive jobs had a greater influence in the shaping of their political opinions and attitudes than their educational training during early childhood.

It may be assumed that the M.P.'s extra-Parliamentary interests exert some influence upon his activities in the House. Some modern politicians have full-time political duties, but others have roots and interests in professional and semi-professional occupations. Many are shareholders and directors of companies, some, owners of private businesses. Until recent times, unless a person had private means his only way of entering Parliament was to exercise some skill which could support him. Thus to-day many Members still belong to occupational groups, such as law and journalism, which permit this practice. The 'ego-involvement', to use G. W. Allport's term, of M.P.'s in these directions may affect their political opinions, and, consequently, the legislation of the country.

The results obtained from the returned questionnaires for these categories are summarized below.

(*a*) *Father's occupation.* Previous investigators of this subject appear to have ignored, to some extent, the importance of the father's occupation in the formation of the M.P.'s

attitude. Prof. T. H. Pear has indicated the importance of social stratification in the formation of attitudes (3). The social stratum to which an individual belongs often depends upon his father's. Some persons sink and some rise socially, compared with their father; but all begin in their father's social class and tend either to absorb or to reject the characteristics of that birth-stratum. It therefore seemed essential to consider this aspect of the M.P.'s social background.

The census-occupation groups cover thirty-two separate categories. Ross used this list in his examination of the previous House. For the present purpose, these have been restricted to twelve categories, as follows:

1. Manual (skilled).
2. Manual (unskilled).
3. 'Blackcoat'.
4. Political (full-time).
5. Titled.

Professions

6. Medicine.
7. Law.
8. Teaching.
9. Journalism.
10. Fighting Forces.
11. Church.
12. Business.

The Labour Members show a preponderance of those whose fathers were skilled manual workers (see Appendix). This is followed by the Blackcoat group. 10% came from Unskilled Manual groups. The Conservatives' fathers were in Business, Blackcoat, Skilled Manual groups, and 14% in the Fighting Forces. Conservatives alone, in our sample, belonged to titled families; these formed 11% of the sample. The fathers of Members of 'Other Parties' came from Skilled Manual and the Blackcoat group, and there were stronger attachments to Medicine and the Church than in the other parties. The Officials' fathers were mostly in Business groups, with a small percentage from Law, Politics, Blackcoat and the Fighting Forces. Only 3% came from the Unskilled Manual group.

This may be summarized: Labour and 'Other Parties' have fathers in Skilled Manual and Professional groups; Officials and Conservatives in Business and Professions. Some idea of the extent to which Members have tended to follow the occupations of their fathers will be gained from this summary of the majority of members in each party sample.

Labour:
Father: Skilled Manual.
First occupation: Skilled Manual.
Present occupation: Politics.

Conservative:
Father: Business and Blackcoat.
First occupation: Armed Forces or Professions.
Present occupation: Politics and Business.

'Other Parties':
Father: Skilled Manual and Blackcoat.
First occupation: Professions and Blackcoat.
Present occupation: Business and Politics.

Officials:
Father: Business and Professions.
First occupation: Professions and Services.
Present occupation: Politics.

(*b*) *First employment.* Three questions seem important in considering the M.P.'s employment: (1) the character of his first job and the age at which he took it; (2) the type of employment he has at present; and (3) whether he began or continued in the same occupational group as his father's. Much of this information does not seem to have been obtained until now. It was discovered, for example, that 65% of our sample had changed their occupation since their first employment. This may be significant in comparing the values of our past and present educational system. The old educational system did not offer higher education on a basis of ability or interest. As a result, many children

sought for other jobs. In the new system this condition may be reduced by tests and the availability of further education for those capable of profiting by it.

The age at which M.P.'s began employment reveals interesting differences between the political parties. A rough grouping indicates that Officials and 'Other Parties' began work after 21; Conservatives between 17 and 21; and Labour below 16. The percentages for the parties are[1]:

	Labour	Conservative	'Other Parties'	Officials
Below 14	32	2	6	3
14–16	31	8	23	21
17–21	18	51	18	33
Over 21	18	33	32	45

In first employment (see Appendix), Labour seems to have begun work equally in four groups: Manual Occupations, Professions, Blackcoat and dead-end jobs. Only Labour seem to have entered dead-end jobs to any significant degree. Conservatives entered, in order of frequency, the Armed Forces, the Professions and Blackcoat occupations. Only 11% began in Manual trades; none in dead-end jobs. Of 'Other Parties', 29% began in the Professions, 26% in Blackcoat, 15% in Manual trades, 6% in the Armed Forces and 3% in dead-end jobs. Of Officials, 40% began in the Professions, 27% in the Armed Forces, 24% in Blackcoat occupations and 9% in Manual trades. We can see again the similarity between the path taken by the Conservatives and that taken by the Officials.

(*c*) *Present occupation.* The present occupation of the majority of the sample appears to consist of full-time political duties (see Appendix). The complete list of occupations held in addition to the duties of the House is too long to be given here, but the following summary gives an idea of the occupations of members in the different parties:

Labour: 58% in full-time Politics, 10% in Business, 14% in Journalism.

Conservative: 41% in full-time Politics, 24% in Business, 14% in Law.

'Other Parties': 26% in Business, 24% in full-time Politics, 21% in Law, 18% in Medicine, 11% in Journalism.

Officials: 79% in full-time Politics; 6% in Journalism.

Amongst groups other than Labour, social status appears to have been maintained or improved by entry to the Armed Forces or the Professions. It is significant that few went directly into political occupations. In all groups there has been a great change of occupation; but in each, the majority, once they have entered the field of politics, have devoted their full time to it.

(*d*) *Business interests.* Of the sample of Labour M.P.'s, 50% claim no business interests; 16% are shareholders in not more than one company; 10% own private businesses. The Conservatives have 62% shareholders in not more than one company; 37% are company directors; 10% are directors of more than one company. In the 'Other Parties', 38% are owners of private businesses, 44% shareholders, 33% company directors. 46% of the Officials have no business interests, and 46% are shareholders in not more than one company.

Compared with previous Houses, a great change has occurred in Members' business interests. In the last Parliament there were 134 company directors in the Conservative Party alone; in this House there are only 33. In past Houses company directors have formed almost one-third of the Conservative Party members (4). In fact, business interests

[1] The figures given refer to a sample of the whole.

the House can now be attributed to only one-eighth of its membership. Amongst the abour Party the Trade Union representation has been diminished to about 9%.

In 1918, 98% of all Labour seats in the House were held by Trade Union officials and s late as 1935 the latter held 50% of the seats of their party. One writer (4) states that ne present House is now made up in the following way:

Labour	
Lawyers, doctors, journalists, technicians, teachers, civil servants	196
Proletarians	77
Trade Union officials	84
Services (professional)	9
Miscellaneous	38
Conservative	
Lawyers, civil servants, journalists, etc.	73
Property, finance and industry	74
Services	22
Miscellaneous	27

(*e*) *Local government.* It appears from our records that few of the M.P.'s who answered is questionnaire have had training in the art of government other than that provided y membership of some local government committee. In this respect the Labour Party's ath is very different from those taken by the other parties. Of Labour M.P.'s 68% have ken some part in local government, in comparison with 30% Conservatives, 28% Other Parties' and 3% Officials. Very few Members seem to have been concerned in ban or rural local government; most have had this service in the large towns or boroughs. any have also been county councillors.

IV. SUMMARY AND CONCLUSIONS

a short article upon a subject presenting very many aspects, it is very difficult to nstruct a succinct summary. The figures presented, though adequate as illustrations, ve little idea of the possibilities presented by the 1946 material. With the help of r G. Furness and of other cooperators, who very kindly are recording these data a Hollerith machine, a thorough statistical analysis is being made of the question- ire replies. This will not only illustrate more vividly the points already mentioned, but ll supply statistics and correlations opening up new avenues of approach.

The introduction suggested that one duty of the psychologist was to record, for purposes comparison, social changes now taking place. The social stratification of the present ouse of Commons is radically different from that of previous Houses. This is apparent nen comparing the present findings with those of Barbara Wootton (who studied the rresponding categories of 1937(7)) and with the more detailed ones provided by Ross (5).

The background of the present House of Commons not only shows greater variety of ucational experience but also of social environment. The Members themselves are gaged in a wide variety of occupations, and their family environments have provided wider range of experience. While it cannot be claimed that every walk of life is repre- nted in the present House of Commons, this Parliament consists of people with a wider nge of environmental conditions than in previous years.

Many of our present M.P.'s began wage-earning at 14, equipped only with an elementary hool education; in this respect resembling the majority of our population. They entered

a wide variety of occupations. Those who left school early, however, proceeded to educa themselves in other ways through political, trade union, church or chapel activitie through attendance at evening classes and by winning scholarships to colleges of no university status. Other members with a similar economic and social environment we able, with the aid of scholarships, to proceed through a university. Our administrator then, do not come exclusively from a small social stratum. Yet it should be remembere that of our total (1945) sample, 35·1% went to public schools and 44·4% attende universities. Our figures show the predominating influence of Oxford and Cambrid in all parties, and the high proportion of M.P.'s educated at Eton. An examinatio of the kind of degrees taken also shows that, in this predominantly scientific age, we a being governed by representatives who studied mainly non-scientific subjects.

The present study concerns the differences of educational and social background the various political parties. Many questions facing the modern politician are scientif and international. How far can a person untrained in scientific method and appreciatio successfully handle or understand problems so intricate as that of atomic power a international standards of nutrition and production? Equally serious are the difficulti presented by to-day's high-tension foreign politics.

Should our M.P.'s be specially trained? Should Parliamentary representation be a fu time job? Should the M.P. of the future 'matriculate' at the poll or be selected by test interviews or examinations before he reaches it? Which sections of our society and whic educational systems at present produce the best politicians? Does the politician whe elected represent his own social stratum ((7), p. 308 and (2)), or is he apt to be absorbe into a higher social group? What common factors lead to a political career—in the sam or different parties—of two people reared in widely different social groups?

At present no scientific method exists which can tease out all the cultural influenc under which any one person has grown up, but the investigation described here h recorded some of the important factors which may tend to shape our rulers and their law It is regretted that the present paper must be confined to a few of the interesting dat obtained from an analysis of the returns, but it is hoped later to publish fuller resul which will interest not only psychologists but sociologists and historians.

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APPENDIX

Father's occupation

Occupation	Labour		Conservative		Others		Officials		Total	
Manual, skilled	165	55%	14	17%	9	33%	3	9%	191	43%
Manual, unskilled	31	10%	0	0	1	4%	1	3%	33	7%
Blackcoat	33	11%	13	16%	6	22%	4	12%	56	13%
Political, full-time	4	1%	5	6%	1	4%	3	9%	13	3%
Professional:										
Medicine	7	2%	2	3%	1	4%	2	6%	12	3%
Law	7	2%	8	10%	2	7%	5	14%	22	5%
Teaching	13	4%	1	1%	1	4%	1	3%	16	4%
Journalism	6	2%	1	1%	0	0	0	0	7	1%
Forces	6	2%	11	14%	0	0	3	9%	20	5%
Church	8	3%	1	1%	1	4%	4	12%	14	3%
Business	19	7%	16	20%	5	18%	8	23%	48	11%
Titled	0	0	9	11%	0	0	0	0	9	2%
Total	299	100%	81	100%	27	100%	34	100%	441	100%

Sixteen not stated.

First employment

Occupation	Labour		Conservative		Others		Officials		Total	
Dead-end job (errand boy, messenger, etc.)	60	20%	0	0	1	4%	0	0	61	14%
Apprenticed to manual trade (mines, farm, engineer)	86	29%	9	11%	5	19%	3	9%	103	24%
Apprenticed to blackcoat trades (shop assistant, clerk)	60	20%	20	25%	9	33%	8	24%	97	22%
Apprenticed to the professions (articled)	75	26%	21	26%	10	37%	13	40%	119	27%
Fighting services	13	5%	30	38%	2	7%	9	27%	54	13%
Total	294	100%	80	100%	27	100%	33	100%	434	100%

Thirty-four not stated.

Present employment

Occupation	Labour		Conservative		Others		Officials		Total	
Full-time, political only	176	56%	36	40%	8	25%	26	82%	246	53%
Manual, skilled	16	5%	8	9%	2	6%	0	0	26	6%
Manual, unskilled	0	0	1	1%	0	0	0	0	1	0
Blackcoat	22	7%	6	7%	1	3%	3	9%	32	7%
Professional:										
Medicine	5	2%	1	1%	0	0	0	0	6	1%
Law	27	9%	12	13%	7	22%	0	0	46	10%
Teaching	14	4%	1	1%	1	3%	0	0	16	3%
Journalism	22	7%	3	3%	4	13%	2	6%	31	7%
Business	31	10%	23	25%	9	28%	1	3%	64	14%
Total	313	100%	91	100%	32	100%	32	100%	468	100%

All occupations of Members and Officials are counted.

We should like to express our thanks to Prof. T. H. Pear for directing this research and
r encouragement and helpful criticisms. The tables were produced by Mr G. Furness.

QUESTIONNAIRE

THE UNIVERSITY OF MANCHESTER DEPARTMENT OF PSYCHOLOGY

NAME .....

ADDRESS .....

DATE AND PLACE OF BIRTH .....

EDUCATION.

Have you attended (state *yes* or *no*)

(1) Private or Preparatory School .....
(2) Elementary .....
(3) Central .....
(4) Secondary .....
(5) Grammar (state which) .....
(6) Technical .....
(7) Public (state which) .....

Have you attended

(1) College (state which) .....
(2) W.E.A. or evening classes .....
State subjects .....
(3) University or Universities .....
State which .....
State Degree taken .....
(4) What scholarships did you win
(*a*) School .....
(*b*) University .....
(*c*) Other .....

OCCUPATION.

At what age did you first start paid employment .....
What was your first employment .....
What is your present occupation(s) .....
What is (was) your father's occupation .....

LOCAL GOVERNMENT.

Have you taken part in local government .....
If so, where and in what capacity .....

BUSINESS INTERESTS.

(Delete those which do not apply)

Are you (1) an employee .....
(2) an employer .....
(3) owner of a private business .....
(4) shareholder in a company or companies .....
(5) company director .....

Signature .....

PUBLICATIONS RECENTLY RECEIVED

Statistical Analysis for Students in Psychology and Education. By ALLEN L. EDWARDS. New York: Rinehart & Co. Inc. Pp. xviii+360. $3.50.

This book is intended to be a first course in statistics for non-mathematical students. The author assumes a bare minimum of mathematical training in his readers and gives, in fact, an introductory chapter explaining some of the elementary rules of simple arithmetic and algebra. No further knowledge is required to grasp and apply the various formulas he presents. The scope of the book is more ambitious than is usual in elementary treatises. In addition to explaining the usual methods of computing means, standard deviations, correlation coefficients, chi-squared, etc., he includes two chapters on the analysis of variance and gives Kelly's 'epsilon' form of the correlation ratio. There is also a useful final chapter on experimental design.

The writing is clear throughout, although the conversational and detailed style, particularly in the earlier chapters, may be irritating to intelligent readers. There is a good selection of exercises, with answers, and the student who works steadily through the book should have little difficulty in learning to compute the varied statistics described. Whether he would also acquire sufficient statistical insight to apply the appropriate methods to particular observational material is another matter. For example, the underlying assumption of normality in most tests of significance is indeed mentioned, but not stressed, and the reader may easily form the impression that most psychological qualities and attributes are in fact normally distributed. Again, in the cause of simplicity, the author works his examples with very small numbers. He constructs a grouped-frequency correlation table, for instance, with only thirty-two cases and performs an analysis of variance with only ten observations. This is rather dangerous for the uncritical student, who may gain the impression that statistical methods are very powerful tools for extracting reliable information from very meagre data. The insistence throughout on reducing the arithmetical labour of computation by 'coding', or working from arbitrary origins, is good.

The book may safely be recommended to elementary students working under the guidance of an experienced statistician. It should be read straight through before beginning detailed study and the discussions and warnings in the last chapter borne in mind thereafter.

The Adolescent in Social Groups: Studies in the Observation of Personality. By FRANCES BURKS NEWMAN. Applied Psychology Monographs of the American Psychological Association, No. 9. Oxford University Press. 1946. Pp. 94. 8*s*.

This pamphlet (which is in multigraph) describes an observational study of between 150 and 200 adolescents of both sexes. The entire study took some eight years. During the course of this period the children were observed in small mixed groups in a variety of situations including lunch-hour picnics, "Clubhouse Living Room", "Counsellor's Waiting Room", "Wimpy's at the Beach", and overnight camping trips.

Great use was made of rating scales in the recording of observed behaviour. These scales were of two kinds: (*a*) *composite*, in which paired traits such as 'talkative-silent', 'peppy-indifferent', etc., were to be rated as 'very marked', 'marked' or 'average' (subsequently rated numerically from 1 to 5); (*b*) *integral*, in which behaviour categories were rated from 1 to 7. The integral scales included such categories as 'Interest in Opposite Sex', 'Grooming Activity', 'Popularity', 'Social Self-Confidence', etc. In assigning ratings, the judges were asked to keep in mind a normal distribution among the scale points. From the ratings made, agreements between raters were examined by calculating product-moment correlation coefficients and 'raising' the resultant coefficients by the Spearman-Brown formula.

The whole of this statistical work is exceedingly dubious. The forcing of trait ratings into a normal scale is most artificial. In what way, for example, can fluctuating traits such as 'talkativeness', 'animation', 'eagerness' and the like, be regarded as normally distributed in a relatively small group of adolescents? Further, the underlying assumption of the Spearman-Brown formula, that errors of observation are uncorrelated, seems most unlikely to be true when the observers form a small group with similar aims and holding frequent conferences to discuss their observations.

In addition, the observers made descriptive records of behaviour which were used to supplement the formal ratings. These were used in attempts to clarify the situation when raters disagreed. It is said (p. 45): "It may be felt that where marked disagreements are possible, it is inappropriate to include the item in question in a rating scale. On the other hand, if we keep clearly in mind that some characteristics are less easily rated than others, and some individuals less easily rated than others, we may find it possible

to use even the less reliable ratings as providing tentative descriptions of behaviour and guides to further observation." How far such considerations were allowed to influence the actual ratings (which, from the method quoted, must have been made retrospectively) it is impossible to gather.

So much for the methods adopted in this study: what of the results? These the reader will hunt for with growing bewilderment. He will find no description of fundamental tendencies in adolescent behaviour, no information that parent or teacher may use as a guide, and no conclusions about the members of the actual group studied. The reader will, in fact, find it very difficult to divine the reasons for publishing this pamphlet.

The writing throughout is prolix and involved to a degree.

Psychology in Infancy and Early Childhood. By ADA HART ARLITT. 3rd ed. London: McGraw Hill Publishing Co. Ltd. 1946. Pp. xiii+475.

Prof. Arlitt's book, already well known and widely appreciated, has been made more complete in its new edition by the inclusion in particular of further evidence and discussion about learning, the higher mental processes and the relations of heredity and environment. Reference to most of the more important research of the last fourteen years is made in the bibliographies. As before, the book remains based mainly upon the results of original experimental inquiries. Nevertheless, it surveys a very wide range of work which it covers concisely, clearly, in an untechnical manner, and with unfailing decision of statement. Readers who have made special studies of particular fields will probably find a tendency to over-simplification and perhaps to dogmatism here and there. But statements are rarely made without evidence, and the good bibliographies make it easy to check up any points that may be considered doubtful. It is in fact a book suitable for both the student and for the general reader and the claim that it presents a picture of the whole development of the normal child is made good.

Mental Disorders in Later Life. Edited by OSCAR J. KAPLAN. Stanford University Press (London: Humphrey Milford). 1945. Pp. vii+436. 30*s*.

For widely understood reasons every large nation is now becoming particularly interested in the physiological, biochemical and psychological problems of ageing. This authoritative and excellent book should, therefore, receive a wide welcome. It is the work of experts, each in his own field, and covers statistical, physiological, psychological, sociological, nutritional, pharmacological, medical and psychotherapeutic fields. In every case the approach is strongly factual, with clear and well-expressed summaries and valuable bibliographies. Naturally the volume is somewhat more specialized than is Cowdray's classical *Problems of Ageing*, but it stands comparison with that mine of information remarkably well, and brings the review down to the most recent work, as well as in many ways focalizing its own particular problems more sharply. The experimental psychologist, interested in the main perhaps in normal developments, can read nearly all of it with great profit. He will, however, be most interested in the two sections dealing with "Physiological aspects of mental disorders in later life", by Dr Nathan W. Shock, and "Psychological aspects of mental disorders in later life", by Prof. Harold E. Jones and Prof. O. J. Kaplan. Both are models of well-selected and clear exposition covering a very wide array of investigations. They do not in any way minimize the difficulties of controlled study in this field. But they do—especially the first of them—tend to bring out the fact that there has been a liability to be a little too prepossessed with a study of *items* in a performance rather than to seek measures of performances within which a variety of items must co-operate. Beyond a doubt this is a volume which should be carefully studied and available for reference by everybody whose interests, for whatever reason, turn in the direction of a study of the phenomena of advancing age.

Psycho-Analysis and its Derivatives. By H. CRIGHTON-MILLER. 2nd ed. London: Oxford University Press (Home University Library). 1946. Pp. 272. 3*s*. 6*d*.

"The present edition", it is stated, has been revised and enlarged. The changes are, in fact, all contained in a series of short appendices occupying seventeen pages in all and in a somewhat extended bibliography. The first of these is called "Obituary" and gives brief but rather bright notes on various leading psychologists. The second is called "Conflicting views" and very briefly shows that psychoanalytic theorizers perhaps do not think in as uniform a way as they once did. The third is an extremely short note on "Introjection". The fourth gives a concise characterization of 'recent trends' in regard to "Suggestion, Therapeutic evidence, Curtailment of treatment, Values, Psycho-physical interaction and synthesis". The fifth glances at Stekel. All the additional material is interesting, but it adds little to the book as a whole, which remains an excellent introductory discussion within its limits.

The State of Public Knowledge. By K. E. Barlow. London: Faber and Faber, Ltd. 1946. Pp. 112. 8*s.* 6*d.*

Public knowledge is, in the main, knowledge of objects mediated by the senses and in particular by the exteroceptive senses. It has invaded almost every field of wide human interest and concern. "The field of knowledge has shifted to include only the atomized physical aspects of materials and their activities. Where such knowledge has been most successful, it has succeeded in eliminating the private variations of knowledge and has concentrated upon knowledge which is shared by all—public knowledge." This thesis is pursued and developed in a number of exceedingly clear and concise essays of a philosophic character, addressed, however, to the well-informed general reader rather than to the professional philosopher. The most interesting part of the book is that which shows how social attitudes, practice and policy, growing out of the acceptance of 'public knowledge' as almost the only kind that is of real worth, may give rise to immense difficulties in the ordering of mankind.

The Warlis. By K. J. Save. Bombay: Padma Publications, Ltd. 1945. Pp. x+280. Rs. 10.

The Warlis are an aboriginal tribe living in the province of Bombay. This account of them and their customs follows a fairly usual anthropological pattern, first presenting some general account of probable origins and history, and then describing in turn various departments of life and interest, such as social organization, religion, marriage, and so on. A considerable number of ritual songs and myths are included, with some indications of their relations to other cultures than that of the Warli themselves. The social psychologist, interested mainly in a study of the deviations and explanations of current social changes, will probably be most attracted by chapters on "Property versus Poverty", "The Drink Problem", "Indebtedness and Forced Labour" and by some concluding remarks suggesting a probable spread of Hindu influences. In fact, in several directions this seems to be a very good 'source book' for the psychologist.

John Keats' Fancy. By James Ralston Caldwell. New York: Cornell University Press (London: Humphrey Milford). 1945. Pp. ix+206. 12*s.*

It seems of considerable interest that from one point of view or another an unusual number of books have appeared lately which discuss invention or construction in thinking and imagination. That is really the theme, applied to the case of Keats, of Prof. Caldwell's book. The sub-title of the book is "The Effect on Keats of the Psychology of his Day", but it seems as if the sub-title is not really meant to be taken very seriously. It is perhaps convenient to be able to say that some of the luxuriant early poems fit best a type of thinking most closely allied to sensational associationism; that in *Endymion*, Keats, using "the intermittent allegory of the dreaming mind", is being influenced by the contemporary psychological interest in the visions of sleep; and that finally Keats began to indulge in "this contemplation of profound opposites", and in this way to find a kind of balance and poise among the products of his freely associating mind. Whether Keats produced his poetry in those ways because he was at all influenced by contemporary psychology seems to matter very little. And it does not matter very much to Prof. Caldwell's book either. The book is, in fact, a critical study of Keats's type of imaginative reaction to his world. It is well written though not always, as befits its topic, easily comprehensible. Most psychologists could read it with great profit, whether or not they make anything of the suggestion that Keats tried to follow an associationistic psychology.

Autonomic Regulations. Their Significance for Physiology, Psychology and Neuropsychiatry. By E. Gellhorn. New York: Interscience Publishers, Inc. 1943. Pp. 373.

Prof. Gellhorn has assembled the experimental facts which show how certain changes in the external environment, mainly of a physiological kind such as anoxia, affect the various organ systems and how the functions of these systems are interrelated. Thus, he describes how the activity of the organism is maintained under conditions of physiological stress. As the title of the book suggests, the discussion is orientated by the principle of homeostasis, that bodily reactions tend to restore the original internal environment.

Familiarity with Cannon's writings, but not with other physiological researches, has occasionally led psychologists to the mistaken view that emotion is distinguished by its concomitant bodily changes, and it is not generally appreciated that every psychological event is accompanied by bodily adjustments, which are mainly accomplished by the autonomic nervous system. The larger part of the book deals with

what may be called the internal part of the response to external stimulation and forms a valuable corrective to undue preoccupation with the external and experiential parts of the response. In his 'emergency theory' Cannon emphasized the sympathetic component of the bodily changes in emotion, but recent researches show that they are less simple and stereotyped than Cannon's researches suggested, and that activity in various parts of the parasympathetic system plays a synergist role. These matters are discussed factually and coherently. The author is to be congratulated on also attempting to show how bodily conditions determine the actions of the organism, although these sections are sketchy and expose the paucity of information in this field. D.R.D.

Emotions and Bodily Changes. A Survey of Literature on Psychosomatic Interrelationships 1910–1945. By Flanders Dunbar. 3rd ed. New York: Columbia University Press (London: Geoffrey Cumberlege). 1946. Pp. lix + 604. 50*s*. net.

No doubt this book, which was first published in 1935, has already found an appropriate place in many libraries. The third edition includes a new introduction of 18 pages, and the addition of 42 titles brings the bibliography to a total of 2400 items. The book's purpose is to introduce to the medical reader those researches which the author regards as the most important ones dealing with the emotional factors in illnesses with bodily manifestations. The selection of researches for description appears to have been unduly swayed by an uncritical enthusiasm for the so-called 'psychosomatic approach', and too little attention has been paid to their scientific standards. In consequence, the book sometimes fails as a reliable guide to the literature. A striking example of this failure is to be found in the new introduction, in which accident-proneness is discussed as a "newly recognized illness syndrome" under the strange heading of "discoveries made through application of psychosomatic principles"; although three pages are devoted to the description of accident-proneness, reference is made only to two papers on experimental neurosis and to two articles by the same author, which share the same defects. Nevertheless, the book contains a large amount of useful information, although to the new edition little of importance is added. D.R.D.

Introduction to Clinical Neurology. By Gordon Holmes. Edinburgh: E. and S. Livingstone, Ltd. 1946. Pp. vii + 183. 12*s*. 6*d*. net.

British psychology owes more to clinical neurology, in personal, technical, and intellectual inspiration, than is easy to assess or possible to repay. Dr Holmes is notable among those responsible for this debt, and a book by him outlining his subject cannot fail to be of interest to psychologists. Unencumbered by clinical and pathological detail, the types of disturbance of function observed by the neurologist are laid out. They are elucidated on the one hand by appropriate methods of examination; on the other by essential anatomical and physiological foundations. The chapters on the motor and sensory systems in particular are distinguished examples of clarity and significance. That upon vision affords some introduction to the fascinating problems of visual disorientation. The difficult subject of eye-movements is most helpfully dealt with. There are equally enlightening chapters on reflexes, convulsions, postural reactions, the autonomic system, and the bladder and rectum. It is only as we come to the speech mechanisms, agnosia and apraxia, and the general questions of mental state that we realize (perhaps with a little sigh of relief?) that there are limits even to the perfection of clinical neurology, and fields to be ploughed afresh. It may be that here the psychologist will find the opportunity of repaying some small part of his debt to John Hughlings Jackson, Henry Head and their colleagues and followers.

The book is not an elementary outline of neurology at large, nor is it intended to be a work of reference. It offers a well-designed background to the observation of both normal and disturbed human nervous function, and as such will assuredly appeal to a circle somewhat wider than that to which it is immediately addressed. R.C.O.

Text-book of Medicine. Edited by J. J. Conybeare. 7th ed. Edinburgh: E. and S. Livingstone, Ltd. 1945. Pp. xvii + 1164. 30*s*.

Most of the more radical revisions included in this seventh edition concern medical problems other than those involved in the study of diseases of the nervous system or of psychiatry. Some of the former section, for which Dr F. M. R. Walshe is responsible, has been rewritten; the latter stands substantially as it did. Both of these sections, however, are remarkably good for the amount of space that can be assigned them, and for these, as no doubt for other sections also, this book is likely to continue its successful career.